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WHAT IS A DOGMA?*

EDITORIAL INTRODUCTION.

The primary significance of a dogma is not its speculative content, but the speculative truth of dogma is expressed in terms of action. Such is the proclamation of a Roman Catholic thinker which has evoked a lively discussion, and although his work has been placed on the Index, this has evidently been for other reasons than any connected with the charge of heresy. For this thesis defines the general concept of dogma in the expressions of the well-known philosophy of action originated by Maurice Blondel and published in his book *L'action* which appeared in 1893, and as far as we know his book was not placed on the Index. "Perhaps," writes Father E. Bernard Allo, O.P., "the thesis sketched by Le Roy is not so different, perhaps the divergencies are less in idea than in expression, in the *significat* itself than in the *modus significandi*" (*Foi et système*, Paris: Bloud et Cie., 180, 181), and this is confirmed by Le Roy himself in a footnote on page 70 of his *Dogme et critique*. A. Houtin in his history of Catholic modernism mentions the Rev. A. D. Sertillanges as expressing the same opinions in the referendum on Le Roy's article on dogma as Father Allo, and so far as we can ascertain, their writings have not been placed on the Index. Further, for a book to be placed on the Index does not mean that it is condemned, but the authorities intend to say that for some reason *hic et nunc* the book is not to be generally read.

This article of M. Edouard Le Roy entitled "Qu'est-ce qu'un dogme?" has even been looked upon with favor in some quarters by representative ecclesiastical authorities; and being of great importance, not only for Roman Catholicism, but also for Protestant-

* Translated by Lydia G. Robinson from the sixth French edition of the author's book *Dogme et critique*.

ism, yea generally for all religion, we take pleasure in rendering it accessible to English readers.

It first appeared in the French fortnightly journal *La Quinzaine* of April 16, 1905, where it was accompanied by an editorial note as follows: "Without expressing any decision on our own part with regard to the opinions of M. Le Roy it seems to us both interesting and useful to take a text from his work by which to invite theologians to furnish the public with the elucidation he asks for. Hence we address a special invitation to all the authorized specialists in Catholic theology, to the professors of our liberal universities and of the larger seminaries, to religious orders, and to the priests."

The invitation was eagerly accepted, and seven later numbers of *La Quinzaine* contained communications of varying importance on the subject. But these formed only a small part of the discussion raised by this striking article. Its publication was followed by a vast array of controversial writings which continued with increasing violence throughout an entire year. Twenty or more other journals opened their pages to the subject; not only such distinctly clerical journals as *Etudes, Revue thomiste, Revue du clergé français, La Croix,* etc., but also general philosophical reviews, *La Pensée contemporaine, Revue de philosophie,* and such liberal journals as *La Justice sociale, Le Peuple français,* and *La Vérité française.* And not only these religious and critical periodicals devoted their pages to the subject but a well-organized opposition to the offending article rushed into print through the daily press.

Still the question which the author put to the clergy in deference to them as being officially charged with the instruction of the people did not receive a satisfactory answer. Many heaped M. Le Roy with malicious calumnies, and many honestly misunderstood him. Many too misjudged him because they knew of the article only through garbled reports or hostile criticisms. He therefore considered it necessary to put the article in permanent form, and so he published it in a book entitled *Dogme et critique* (in the series *Etudes de philosophie et de critique religieuse* with Librairie Bloud et Cie.) together with his published replies to the most important of his adversaries, a careful bibliography of the controversy and a more detailed development of the most significant points of his thesis in fourteen brief additional chapters.

* * *

Religion is a practical affair, and its main purpose is to serve

us as a guide through life. Religion as a sentiment is practically universal and we may consider it to be innate. It is a panpathy or all-feeling which produces in every individual a deep-felt longing to be at one with the whole universe of which each is a part.[1] As every material particle is an embodiment of gravitation in proportion to its weight, and is possessed of a well-apportioned pressure somehow and bent some whither, so the souls of things existent feel themselves parts of the great whole in which they live and move and have their being.

This panpathy in its historical development under definite conditions assumes a definite form, and so religion leads necessarily and naturally to church life and church formation, with dogmas and regulations of conduct.

The dogmas of the church are collected in what has been called the symbolical books which accordingly contain the several confessions of faith. They are called symbolic because they served as symbols, or tokens of recognition to the members of the church. The man who could recite the symbol was welcome in the congregation as a brother who cherished the same faith, having found the same solutions of the world problem as the whole church and having accepted the same formulation of it.

The dogma is a symbol, but it is more than a symbol; it is an appropriate symbol. It is a statement satisfactory to the whole congregation and in so far as it is satisfactory to the whole congregation it has become to them a truth.

Dogmas are truths. Being religious truths they are holy truths, and since they are taken seriously, they have often become the cause of much controversy and have led to quarrels and bloodshed, to persecution and warfare, to the establishment of the inquisition and denunciation of heretics. We now learn that the intellectual feature of the dogma is derived from the main and essential feature, its practical value. This is an enormous gain, for it introduces into the nature of dogma a condemnation of all intolerance and establishes an unlimited freedom of interpretation without, however, detracting a hair's breadth from the practical significance of the dogma. Not one jot or one tittle shall pass from it, but a thinker is allowed to construct its meaning as best he can, provided he recognizes and holds on to its practical application.

God is our father; he is called upon in prayer as a personality

[1] For a more complete definition of religion in its several phases see Carus's *Dawn of a New Era*, pp. 96-97.

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—not a human personality, but a divine personality. The interpretation of personality is a problem by itself, but the significance of the dogma "God is a person" means that we should adjust our relation to God in such a way as to make it a personal relation, and this practical application constitutes the primary and underived significance of the dogma.

This view is not a loose way of treating the dogma; for the freedom of interpretation gives much liberty of speculation, but not an unlimited license. It is restricted and allows the dogma to stand and remain unalterable as the only possible, the only allowable, expression of a truth. Though the dogma is not absolute it is definite, and any other formulation of it would be wrong and must be rejected. Thus the view of dogma here represented by M. Le Roy remains as uncompromising as ever and would not allow any dillydallying for the benefit of speculative minds.

It will be sufficient to characterize the author's effort and the misunderstandings created in the broad problem in his own words. They will show first the sincerity of his undertaking and explain the situation of his own mind, and secondly they will describe his critics and their inability to grasp M. Le Roy's point of view. A faithful Catholic's understanding of the nature of dogma is characterized by the article itself and for a summary of this phase of religious thought it is fully sufficient.

This is what our author says in speaking of himself (*Dogme et critique*, pp. v-x):

"On April 16, 1905, I published in the *Quinzaine* an article entitled 'What is a Dogma?' in which, speaking as a philosopher who desires to think his religion, I addressed various questions to theologians and apologists.

"Why did I use the form of interrogation instead of a direct exposition? In deference to those who have official charge of instruction. It seemed to me desirable that the reply should come from them. In this way I hoped to manifest my intention to act always in conformity with the hierarchical principle divinely established in the church. Although I have scarcely been able to congratulate myself on the reserve and courtesy I thus showed, since some have been pleased to see in it only a caution lacking in courage and candor, still I retain to-day the same way of looking at things. But be assured this does not in the least mean that I experience the slightest difficulty for my own part in reconciling

faith and reason, nor that I hesitate or doubt the least bit in the world with regard to my duty as a Catholic.

"My aim was, briefly, to expose certain *facts* which I had had the opportunity to observe around me, and also to report an *experience* I had had in my relations with the unbelieving intellectual world. It was for the theologians, I thought, to declare themselves after discussing the plan which I submitted to them. As for myself, I was only a witness testifying to what he had seen and come in touch with, a Christian soul relating some of the steps it had taken.

"This attitude has been misunderstood. It has been regarded as craftiness or malice, as a challenge or an irony. Some one spoke with reference to it of a question 'irreverently and even impertinently stated.'[2] Was not 'importunately' meant instead, without daring to say it, or admitting it? For, I beg to inquire, how may one set about being more deferential than I have been? Unless the only deference that is acceptable and sufficient is the deference of an indifferent or heedless silence. Is it true that the question asked was indiscreet? Certain papers hastened to make the claim, and the *Siècle* for instance was much diverted at the idea of Catholics not being able to agree on defining a dogma. These are certainly not my own sentiments. In asking an explanation I never intended to be, nor do I think I was, a trouble maker, disturbing slumber or ruffling tranquillity. But words like those I have mentioned tend to justify this ill-natured hypothesis, and therefore it is they which in the final analysis I find lacking in courtesy.

"For my part, on re-reading what I have written and feeling ready to write again, I declare with M. Fonsegrive:[3] 'Have we been wrong in saying these things out loud and, being Catholics, in having enough confidence in our religion, in the power of truth, to dare speak frankly, clearly, even vigorously? Would we have shown more regard for our beliefs if we had spoken timidly and feebly as one speaks at the bedside of the dying?' One must indeed stand up for oneself. We are neither dissembling Protestants nor disreputable rationalists. We are only searching always for the greatest religion, without concessions or haggling. We do not wish in the least to be either rebels or even eccentric persons. But our faith is firm enough for us not to fear to look the facts in the face and to speak out clearly what they show us; and we attach enough

[2] *La vérité française*, Dec. 20, 1905. [3] *Quinzaine*, Jan. 1, 1906, p. 30.

value to the divine word to wish to think with all the strength of our soul, assured in advance that there we will find life and light without other limitations than our own. Moreover we feel that we are enough protected by the living supremacy of the church to preserve the most complete internal peace throughout our most venturesome inquiries. We are, in fine, sure enough of our obedience to legitimate authority to have no fear in running the commendable risks which the experience of life always entails. But the obedience we intend to render is not a simple obedience of formulas and motions, it is a profound obedience which lays hold of our whole being, heart, will and intelligence—in short, an obedience of reasonable men and free agents, not of slaves or mutes.

"Nevertheless, as soon as the article 'What is a Dogma?' appeared a vast array of controversial writings began which continued with increasing violence during one whole year. Not only did the reviews take part, as was their natural business, but the daily papers as well. For after having reproached me for opening a discussion on such a subject before a public which though educated was not professionally qualified, they had nothing more urgent to do than to force the discussion before the eyes of a crowd which this time had neither proficiency nor culture. The organization of the exposure was perfect and the matter was abundantly exploited by those who make orthodoxy a monopoly or a standard and who are always to be found upon the heels of any one who takes the liberty of thinking for himself.

"To polemics conducted in this way I shall make no reply. Their authors, in spite of the pretensions they parade, are representative of nothing in the church, and as, on the other hand, they do not discuss but condemn and anathematize, substituting injury, slander or denunciations for arguments, they are representative of nothing from the intellectual point of view. What separates us from them is a question of morality much more than a question of critique.

"Fortunately other questioners have made their voices heard, loyal and disinterested questioners of broad minds and upright hearts, striving to understand and seeking nothing but the kingdom of God, the welfare of souls, the light of truth. The present volume is dedicated to them, to them and all those, whether known or unknown, who are like them. Is there any need of justifying oneself otherwise than by the words of Fenelon, which he might have taken

for a motto: 'Every Christian, far from entering controversies, ought instead to explain his position more and more to try to satisfy those who have had trouble with the first explanation.' If this motive is not sufficient I may add that I cannot remain indifferent in the face of the opinions that have been attributed to me. Too many people have become acquainted with my article only through incomplete analyses, through prejudiced reports or through refutations which may well confuse them; it is important that I should publish an authentic text with comments made necessary by the publicity the controversy has attained.

"For the rest, I still retain the same attitude I had at the beginning. I wish to put a question, nothing more. The accompanying comments and reflections are only to elucidate the meaning and the scope; to show also that it has not in the least been adequately answered; finally to furnish a definite theme for discussion and investigation. Who would dare to find occasion in this to accuse me of heresy?

"And now I have finished my task on this point; I have said what I had to say. The question has been asked, and nothing could prevent it from being asked. Henceforth the ideas will make their way of themselves and nothing will stop them. Let the future answer. Perhaps we shall soon see what has often happened before, that what was once regarded as bold and disgraceful will end by being universally accepted as a very simple and commonplace matter."

* * *

According to Le Roy the intellectual feature of the dogma is not denied nor abrogated. On the contrary it remains in force and takes about the same place in religion as the laws of nature in natural science which formulate uniformities of facts but are not the actual phenomena as experienced. They both have their positive significance. It seems to we that in this way this conception of the dogma is helpful to educated people.

It is not necessary to make the interpretation of religion become a product of the Aristotelian philosophy. It would change theology into an *ancilla* of medieval thinking and deprive it of the liberty to adopt the scientific spirit.

While Le Roy's theory resembles pragmatism, one cannot characterize it as purely pragmatic, and we should consider that the papal decree, *Lamentabile sane exitu* of July 3, 1907, condemns the

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views of those who claim that dogma is exclusively a *regula praeceptiva actionis,* and that it is not a *regula fidei.* Nor is Le Roy an agnostic. He positively affirms that we can know God in relation to ourselves, and also that we can know him as he is *in se.* The essence of the dogma according to him is not exhausted in its moral significance, but includes also the *enunciatio speculativa.*

The distinction between the actionists and analogists is more one of words than of actual meaning, for both agree in presenting the truth concerning God in terms of intellectual conception and in terms of action, and thus both sides insist on a real cognition of God, each in his own terms. The whole controversy turns on this question, "Is practical truth contained in the speculative, or the speculative in the practical?" while we might say they are both two phases of the same. P. C.

THIS title, "What is a Dogma?" is only a simple question and by no means does it promise an answer. It is a question from the philosopher to the theologian calling for an answer from the theologian to the philosopher.

It would indeed be vain to pretend to give here a complete and definite answer to this complex question. Such problems cannot be solved in a few pages. Therefore the reader must not look for a settled doctrine in the short article which is to follow, nor even for categorical theses on any point. If he sometimes find that I speak in too affirmative a tone let him be kind enough to admit that I do so only for the sake of greater clearness in my questions. In fact I wish to confine myself to simple suggestions which I present merely as rough drafts of solutions offered for the criticism of those who have authority to judge of the subject. And moreover I can justify this attitude of mine by an imperative reason, namely that I am not a theologian and do not like to decide matters in which I am not proficient.

Perhaps some one will ask, why then do I take the

trouble to treat a subject of which I admit I have no particular knowledge? Here is my reason. In our day every layman is called upon to fulfil the duty of apostleship in the incredulous world in which he lives. He alone can serve efficiently as the vehicle and intermediary of the Christian message to those who would not trust the priests. Therefore it is inevitable that some problems of apologetics should be laid before him, problems whose solution is an absolute necessity for him if he does not wish to fail in the task which the force of circumstances has laid upon him without possibility of escape, if he wishes to be always ready, following the counsel of the Apostle, to satisfy those who ask him the reason for his faith. It is only natural therefore that I desire to be informed; and if I formulate my question publicly it is because I am not the only one in this situation, and because there is a general interest that the answer shall also be a public one.

Besides I have another motive for acting as I am. If I freely acknowledge my incompetence in a matter which is properly theological, yet on the other hand I consider that I am well situated to appreciate correctly the state of mind in contemporary philosophers that is opposed to the understanding of Christian truth. And it is to this that I bear witness in saying frankly, even brutally (if I must in order to be fully understood), what I know, what I have observed, what perhaps are not always sufficiently comprehended, namely the exact reasons why unbelieving philosophers of to-day repulse the truth that is brought to them, and the legitimate causes (agreeing in this with the Christian philosophers themselves) why they are not satisfied with the explanations that are furnished them.

My ambition goes no farther than to point out certain opinions, perhaps to suggest certain reflections, especially to particularize the statement of certain problems. If the present work bring a useful contribution to the studies of

religious philosophy, if it furnish documents and materials which others can turn to account, I shall have attained my end. It is not a question of upholding a system nor of aligning arguments for or against this or that school, but only of elucidating certain fundamental ideas whose consideration is imposed upon every system and upon every school. An effort toward light in the bosom of Catholic truth, faithfully accepted in its completeness and rigor—this is what I submit to the decision of those who have been charged with the duty of defining and interpreting it.

What I desire above all, I repeat, is to make better known the state of mind of those contemporaries who think, the nature of the questions they ask themselves, the obstacles that hinder them and the difficulties that perplex them. It cannot be denied that the classical replies no longer satisfy them; there is no use in disputing over so obvious a fact. The experience of cultivated non-Christian circles (I might even say a personal experience) has demonstrated to me that the proofs brought forward as traditional have no effect on intellects accustomed to the discipline of contemporary science and philosophy. Now why this new impotence of old methods which have sufficed so long? The reason appears to me to be, at least in great measure, that the old apologetics assumes the greater part of the problems to be solved in advance which the moderns, on the other hand, judge to be essential and primordial. The real difficulty for the moderns comes in altogether before the arguments begin by which the theologians flatter themselves they can convince them; it lies in the postulates taken for granted and in the very manner in which the investigation is approached.

It will be well to see how the questions ought to be put to-day; this should be the first result to be obtained. It is the chief result, for without it we would never arrive at anything serious. Thus is imposed the preliminary task

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of coming in contact with the minds whom one wishes to address and whom one claims to understand. It is necessary that the various chapters of the apologetic should be taken up successively from this point of view in order to be brought to general attention; and in examining here the idea of dogma[1] I only give a first example of the kind of work that I think ought to be generally undertaken.

Let no one think such a task profitless or superfluous. On the contrary, nothing is of greater urgency to-day nor of more pressing necessity. It is strange and lamentable how little we on the Catholic side know or how greatly we fail to appreciate the state of mind of the opponents to whom we try to speak.[2] Nor are we listened to or understood. What we say has no response and carries no weight. We exert ourselves in silence and in a void without even giving rise to any criticism or refutation. In short we only reach those who do not need to be reached—I mean those who are convinced beforehand or whose difficulties are not of a theoretical kind. We must not deceive ourselves. Catholic thought at the present day is without notable influence on the various intellectual movements which are developing around us. It sometimes follows them at a distance and after having resisted them for a long time; but nowhere does it appear capable of directing them, much less of promoting them. There is nothing more sad than to confess so many efforts expended without result on the one hand, and on the other hand so many sincere questions asked which remain unanswered.

Doubtless one might say, and indeed some have said, that there is no need of taking into account modern demands because they proceed from a perverted and misguided judgment. Wretched subterfuge! What contemporaneous thought is asking for beyond what it receives

[1] I will say once for all that by "dogma" I mean especially the "dogmatic proposition," the "dogmatic formula," not at all the reality which underlies it.

[2] I would say the same, moreover, of our opponents with respect to us.

is perfectly legitimate, and there is no justification in pretending to refuse to grant it. Men of to-day are within their rights in not consenting to be held down to the point of view of the thirteenth century. It would indeed be strange if any one should ask for a proof to support a truth of this kind.[3] After all, is it not the very mission and the *raison d'être* of apologetics to address itself to the disordered, if such there be? It must take people as they are and not require of them that they first come of their own accord where it may prefer. Once again, it would be strange if one had no right to make a cure except with certain remedies.

Hence there may be some interest and some profit in the testimony of those whose situation has put them in a position to know the modern mind, its needs and its requirements. These may try to tell how they have come to think what they believe, how they have succeeded in practically overcoming, and of their own accord, the difficulties that they have met like the others. I do not say that we must accept the conclusions of their experiences uncritically; but after all, these experiences offer the advantage of furnishing living documents, not dead opinions, and that is something. I here make no further claim.

One more word before I begin. Perhaps the reader will be surprised to find so long a preamble introducing so short an article. The reason is first of all that I wished to write a sort of general preface for other similar articles intended to follow this one, and also because I wished in this way to forestall any possible misunderstanding. Whatever opinion may be held on the ideas which I shall put forth, it must not happen that any one will try to answer me by charging me with heresy. I affirm nothing in this work except facts easily verifiable by everybody. As to

[3] The object of faith always remains the same but not the manner of thinking it or of complying with it.

the rest, that is to say the sketches of theories, whatever the form of the language which I have adopted in order to make myself clear, I give them expressly as simple *interrogations* addressed to whomsoever they may concern. In a word, I do nothing but state some problems; it is for the apologists and theologians to solve them.

* * *

We no longer live in the day of partial heresies. Formerly a purely logical and dialectic argumentation might suffice because certain common principles were always admitted on both sides. But the case is no longer the same to-day, when these principles go by default, when the fundamental difficulty is to establish a point of departure upon which both sides may agree. To-day denial does not attack one dogma any more than another. It consists above all in a preliminary and total demurrer. The question is not whether a proposition is a dogma or not; it is the very idea of dogma which is repugnant, which gives offense. Why is that?

When we examine the ordinary motives of this repugnance we find four principal ones which I shall briefly enumerate, endeavoring to present them in all their force:

1. A dogma is a statement presented as being neither proved nor provable.[4] Those who declare it to be true declare at the same time that it is impossible ever to arrive at the point of grasping the intimate reasons of its truth. Now modern thought, faithful to the precept of Leibniz, endeavors more and more to demonstrate the old so-called axioms. At least it wishes to justify them with Kant by a critical analysis which shows them to be necessary conditions of consciousness implied *a priori* in every act of reason. It is distrustful of those evidences, pretending to be direct, which were so numerous in former times. Often enough it discovers in them simple postulates adopted for

[4] I mean here to speak of *intrinsic* proof.

an end of practical utility more or less unconsciously perceived.[5] In short everywhere and always it calls for long and detailed discussions before believing itself authorized to draw conclusions. And it is not just any more or less roundabout proofs that it thus demands, but direct specific proofs. It does not like too general arguments which look upon vast assemblages as a whole and proceed by wholesale demonstrations, because it has had experience too many times with the illusions, mistakes and oversights which they ordinarily conceal. Nor does it like any better external, extrinsic arguments which end in proofs of a negative character, in *reductiones ad absurdum* founded on judgments of contradiction or impossibility, because it has also had experience[6] too many times with their imprudent and hazardous character to declare either impossible or contradictory a thing which may appear so to us only from habit. Therefore it seems that in order to remain faithful to the tendencies which have assured its success in all domains modern thought can do no less than condemn absolutely the very idea of a strictly dogmatic proposition. In what system acceptable to reason could such a proposition find room without violence? Is not the first principle of scientific method incontestably, according to Descartes, that it must hold as true only what clearly appears to be true? What justification would there be for making an exception of just those propositions which pass as the most important, the most profound and the simplest of all? When affirmations are of the greatest consequence and refer to the most difficult and recondite subjects it is certainly not fitting to show oneself less attentive to the exactness of the rules which constitute our protection against error. On the contrary it is just then that it would

[5] Compare the *Philosophie nouvelle* edition of Bergson's works.

[6] Especially in the sciences.

be legitimate to be even more exacting, more scrupulous, more particular than usual.

2. It will doubtless be said that dogmatic propositions are never affirmed without proof. In fact an indirect demonstration has been attempted over and over again. One certain apologetic which is regarded as purely traditional[7] claims to prove that these propositions are true, although it realizes that it is incapable of bringing fully to light the how and the why of their truth. There is some analogy, it seems, between such a proceeding and that of the mathematician who limits himself at times to the theorems of simple existence, or that of the physicist who often accepts facts of which he cannot give any theoretical explanation, or yet again of the historian who always receives knowledge only by the path of testimony. Thus would end the first objection.

Yes, here we would have a very simple solution, but there is one misfortune, namely that the analogy pointed out proves upon reflection to be absolutely inaccurate. The difficulty we wish to avoid reappears *in toto* when we try to justify postulates on which the alleged indirect demonstration rests. When a mathematician is satisfied with establishing a theorem of simple existence, I mean a theorem affirming the existence of a solution inaccessible in itself, he reasons no less rigorously than in other branches of his science. Now here we have nothing like that. It would be necessary to prove *directly* that God exists, that he has spoken, that he has said this and that, that we possess his authentic teachings to-day. This amounts to the same thing as saying that the problem of God, the problem of revelation, of the inspiration of the Bible and of the authority of the church, must be solved by a *direct* analysis. Now these are questions of the same kind as the strictly dogmatic

[7] This method of *extrinsic* demonstration is regarded as traditional. Here is a historical point on which much might be said, but such a discussion is foreign to my subject.

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questions, questions with reference to which it is indeed impossible to produce arguments comparable to those of the mathematician. Likewise when a physicist accepts a fact to which he can give no theoretical explanation this fact corresponds, at least for him, to certain definite experiences, to certain manipulations that can be practically carried out, in short to a group of motions of which he has direct knowledge. What similarity is there here? And finally even the historian does not consent to receive truth by testimony except because he is dealing with phenomena of the same kind as those of which he has a direct view by some other means. He still regards his science as always conjectural and uncertain so long as it treats of somewhat profound causes or of events that are more or less remote. How much more ought one draw the same conclusion in the case of dogmas which reflect only facts that are mysterious, strange and disconcerting, and to which no analogy in our human experience corresponds! It has been well done. The alleged indirect proof has inevitably for its basis an appeal to the transcendence of pure authority. It claims[8] to introduce the truth into us fundamentally from the outside in the fashion of a ready-made "thing" which might enter into us forcibly. Thus any dogma whatever seems like a subservience, like a limit to the rights of thought, like a menace of intellectual tyranny, like a shackle and a restriction imposed from without upon the liberty of investigation—all of which is radically opposed to the very life of the spirit, to its need of autonomy and sincerity, to its generative and fundamental principle which is the principle of immanence.

Let us insist a little upon this last point, for the principle of immanence has not always been rightly understood. Too often it has been made out a monster, whereas nothing

[8] Or at least appears to claim, which is the form under which it is too often presented.

is more simple nor on the whole more clear. We may say that to have gained a clear consciousness of it is the essential result of modern philosophy. Who refuses to admit it is from that time forth no longer counted among the number of philosophers, who does not succeed in understanding it indicates thereby that he has not the philosophic sense. And this is what constitutes the principle of immanence. Reality is not made of separate pieces put in juxtaposition, but everything is within everything else; in the smallest detail of nature or of science analysis recognizes all of science and all of nature. Each of our states and of our actions comprises our entire soul and the totality of its powers. Thought, in a word, is wholly included in each of its moments or degrees. In short, there is never for us a purely external fact like some sort of raw material. Such a fact indeed would remain absolutely unassimilable, unthinkable; it would be a nonentity to us, for where could we take hold of it? Experience itself is not in the least an acquisition of "things" which previously were entirely unknown to us. No, it is much more a transition from the implicit to the explicit, a profound movement revealing to us the latent requirements and actual abundance in the system of knowledge already explained, an effort of organic development, putting to use its reserves or arousing needs which increase our activity. Thus no truth ever enters into us except as it is postulated by that which precedes it as a more or less necessary complement; just as an article of food to become valuable as nourishment presupposes in the one who receives it certain preliminary dispositions and preparations, for instance, the appeal of hunger and the ability to digest. In the same way the statement of a scientific fact presents this character, no fact having meaning nor, consequently, existing for us except by a theory in which it is born.

On these various points a critical examination of the

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sciences has recently come to confirm the reflection of the philosophers. It is obvious that I could not enter here into detail,[9] but the little that I have said will doubtless suffice to give a glimpse at least of how that which has been called *extrinsicism*[10] is opposed in spirit, attitude and method to modern thought.

3. In spite of what we have just said let us admit, however, the instruction of dogmas by simple affirmation of a doctrinal authority which is accepted almost without criticism. Nevertheless, in order to be acceptable these dogmas would need to be perfectly intelligible in their statements, leaving no room for any ambiguity of interpretation or any possibility of error with regard to their real meaning. Now this is not the case. In the first place their formulas often belong to the language of a particular philosophical system which is not always easily understood, which does not always escape the danger of equivocation or even of contradiction. There is no doubt, for instance, that the doctrine of the Word in origin and context is closely connected with Alexandrian neo-Platonism; that the theory of substance and form in the sacraments and that of the relations between substance and accidents in the dogma of the real presence, are really closely connected with Aristotelian and scholastic conceptions. Now these diverse philosophies are sometimes doubtful as to their basis and obscure as to their expression. In any event they have long been antiquated, fallen into disuse among philosophers and scholars. Would it therefore be necessary, in order to be Christians, to commence by being converted to these philosophies? This would be a difficult undertaking, before which

[9] See the *Bulletin de la société française de philosophie,* meeting of February 25, 1904.

[10] Blondel uses the term *extrincésisme* together with *historicisme* to denote two kinds of apologetics which he condemns. See his article on "Histoire et dogme" in *La Quinzaine* of Jan. 15, Feb. 1 and Feb. 15 of 1904.

many believers themselves would feel strangely embarrassed. And moreover even this would not suffice, for the confusion of many languages resulting from heterogeneous philosophies constitutes still another difficulty no less troublesome than the first.

But this is not all. Aside from this, dogmatic formulas contain metaphors borrowed from every-day matters, for instance when they speak of the Divine Fatherhood or Sonship. It is impossible to give an exact intellectual interpretation of these metaphors, and consequently to determine their precise theoretical value. They are images which cannot be converted into concepts. It would require anthropomorphism to take them literally, and at the same time it would be difficult to give them any deep significance. One cannot even handle them without reserve, nor follow them to a conclusion without arriving too quickly at ridiculous consequences and absurdities. Hence arises a great uncertainty that continues to increase the confusion of imaginative symbols with the abstract formulas of which we were just speaking.

After all, the first difficulty with regard to dogmas which many people find to-day consists in the fact that they do not succeed in discovering a thinkable meaning in them. These statements tell them nothing, or rather seem to them to be indissolubly connected with a state of mind which they no longer possess and to which they think they are no longer able to return without degenerating. Moreover many believers are virtually of the same opinion, and prefer to refrain from all reflection, foreseeing certain obstacles that they would meet in thinking what they believe under the forms laid before them. A contemporaneous philosopher has said: "What would most embarrass the greater number of believers would be if, before asking them for a *proof* of what they believe, one were simply to

call upon them to *define* exactly what it is they *affirm* and what they *deny*."[11]

4. Finally, let us pass over these difficulties. Even after they are disposed of there still remains a last objection which seems very grave, namely that in any event dogmas form a group incommensurable with the whole of positive knowledge. Neither by their content nor by their logical nature do they belong to the same system of knowledge as other propositions. They therefore could not be arranged with others in a way to form a coherent system, so that if one accepts them the result is an inevitable breach of unity in the mind, a disastrous necessity of playing a double part. Being unalterable they appear foreign to progress, which is the very essence of thought. Being transcendent they exist without relation to effective intellectual life. They bring no increase of light to any of the problems which occupy science and philosophy. Thus the least reproach that one can cast upon them is that they seem to be without profit, to be useless and barren—a very grave reproach in a period when it becomes more and more perceptible that the value of a truth is measured above all by the services that it renders, by the new results that it suggests, by the consequences which it brings forth, in short by the vivifying influence it exerts on the entire body of knowledge.

Such, briefly summed up, are the principal reasons why the idea of any dogma whatever is repugnant to modern thought. I have endeavored to present them in all their force, taking the same point of view in setting them forth as those who regard them as conclusive and speaking, so to say, not in my own name but in theirs. It remains now to investigate some conclusions and some lessons which we ought to be able to derive from them.

[11] Belot, *Bibliothèque du congrès international de philosophie de 1900.* Paris: Armand Colin.

These reasons, it must be recognized, are perfectly valid. I do not see any legitimate way of refuting the preceding line of argument.[12] The principles which it invokes seem to me no more contestable than the deductions which it draws from them. In fact I do not see that it has ever been answered except by worthless subtleties or rhetorical artifices.[13] But eloquence is not a proof, neither is diplomacy. Hence our only real resource is to prove that the idea of dogma which is condemned and rejected by modern thought, is *not* the Catholic idea of dogma.

Perhaps it will be found that in speaking in this way I depart from the role in which I have promised to confine myself, that this time decidedly I am stating theses and not asking questions. This would be a mistake. There is no doubt that I am affirming something here, but what? Nothing but *facts*. It is a fact that the unbelievers of to-day are halted in the face of dogmas by the foregoing objections. It is also a fact that whoever (even among believers) has truly comprehended the spirit and the methods of contemporary science and philosophy, cannot but give his assent to these objections. Now please note: those very people who submit most completely and most cordially to the authority established over them could not be affected by it. No authority indeed could bring it about or prevent that I find an argument valid or weak, nor especially that this or that notion has or has not any meaning for me. I not only say that no authority has any right in the world to do so, but that it is absolutely impossible; for after all it is I who do the thinking and not the authority that thinks for me. No argument could prevail against this fact. I can neither force myself to feel satisfaction nor prevent myself from feeling it at the evidence on one side or an-

[12] I say refuting, but it could be *cut short* by destroying the postulate which is its root.

[13] It would be interesting to enter into a detailed discussion of these answers, but there is no room for it here.

other. To be sure I admit that authority imposes upon me this or that belief with the result that it makes me follow this or that line of conduct, but how could it compel me by virtue of such a proof to believe what I do not regard as convincing? And how would I be able to obey it if it commanded me to understand this or that declaration which I did not understand at all? As well might it require me to cease thinking. No *reason* can be founded on faith. Here we have an *identity* pure and simple. There is no such thing as *revealed logic*.

Hence I come back to what I said a while ago, and, *speaking as a philosopher,* I declare myself incapable of thinking differently from our adversaries on the above-mentioned points.

Moreover in making this declaration I consider that I am doing nothing but stating a problem. The state of mind which I have described exists, it is triumphant to-day; even those who believe the most firmly share it. These are the *facts* which it is impossible not to take into account and which constitute, I repeat, the statement of a question to be solved. Let us see exactly what this question is.

I shall henceforth regard it as granted that the objections summed up above cannot be evaded so long as the idea of dogma which they contain is preserved. Does this mean that we must conclude definitely that there is an absolute incompatibility between the idea of dogma and the essential conditions of reasonable thought? That in order to think as a Christian it is necessary to cease thinking altogether? I certainly do not believe so. But to avoid the objections in the case and to obtain the desired harmony I ask myself if it is not the very manner in which the idea of dogma is presented that is the real cause of the contention, and if consequently we have not reason to change this manner.[14]

[14] I beg the reader to give heed to the limits within which this question is

Now when we examine the conception of dogma which the four objections above enumerated assume and imply, we are surprised to find that it is common to the greater number of Catholics and their opponents. It is a distinctly intellectual conception. It regards the practical and moral meaning of the dogma as secondary and derived and places in the first rank its intellectual meaning, believing that this constitutes the dogma whereas the other is merely a consequence of it. In a word, it makes of a dogma something like the statement of a theorem—an intangible statement of an undemonstrable theorem, but a statement having nevertheless a speculative and theoretical character and relating above all to pure knowledge. This is the common postulate that one discovers by analysis at the foundation of both of the two opposed doctrines, the one that accepts and the one that rejects the idea of dogma. Here I believe is the crux of the difficulty. From this unexpressed postulate and from the conception which flows from it originate, in my opinion, both the abuses to which the idea of dogma can give rise and the conscientious objections that it raises. Indeed it is inevitable that one would finally draw the conclusion that all dogma was illegitimate, for he would at the same time define it as a theoretical statement while nevertheless attributing to it characteristics the very opposite of those which make statements correct. It is very curious that the apologists are not more often informed of a fact of such great importance as that their conception of dogma would destroy in advance the theses that they wish to establish. On the other hand, the same intellectualist idea of dogma leads to two very regrettable and unfortunately very frequent exaggerations; one consists of confusing dogmas properly so called with certain opinions

comprised. It does not discuss in any way the modification of the content of dogma, nor even its traditional religious interpretation, but only the determination of the modality of the dogmatic judgment and of the qualification it possesses.

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and certain theological systems, that is to say, with intellectual accessory representations; the other, in failing to see that a dogma could never possess any scientific significance and that there are no more dogmas concerning for instance biological evolution than there are concerning the movements of planets or the compressibility of gas.

From a thorough study of these various points we reach the conviction that the problem of dogma is usually badly stated;[15] and perhaps we will see at the same time how it ought to be stated in order to render possible a satisfactory solution.

* * *

From this point I enter at once into the domain in which I must keep myself in an interrogatory attitude. This is my definite intention although to insure clearness I may keep the didactic tone. What follows must be taken as a simple exposition of what I ordinarily reply to those who ask me what I think of the idea of dogma. Am I wrong to speak in this way? I am quite ready to acknowledge it if any one will show me that it is not the right way in the eyes of the church.

First of all I say that a dogma cannot be compared to a theorem, of which we only know the statement without its proof and whose proof can only be guaranteed by the assertion of a teacher. Nevertheless I know that this is the most common conception. We like to think of God in the act of revelation as a very wise professor whose word we must believe when he communicates to his audience results whose proof that audience is not capable of understanding. But this appears to me to be hardly satisfactory. We say that God has spoken. What does the word "speak" mean in this case? Most certainly it is a metaphor. What is the reality which it conceals? Herein lies the whole difficulty.

[15] At least in books in current use and in elementary education.

Without recurring to the general considerations I have already developed let us take some examples that will serve to specify what we have hitherto looked upon only in large outlines.

"God is a person." Here we have a dogma. Let us try to see in it a statement having above all an intellectual meaning and a speculative interest, a proposition belonging first of all to the order of theoretical knowledge. I pass over the difficulties aroused by the word "God," but let us consider the word "person." How must we understand it?

If we grant that the use of this word bids us conceive the divine personality in the form shown to us by psychological experience on the model of what common sense designates by the same name, as a human personality, idealized and carried on to perfection, we have here a complete anthropomorphism, and Catholics would certainly agree with their opponents in rejecting such a conception. Moreover to carry such a thought to its extreme limits is a very delicate thing, very likely to induce error or at least mere verbiage, incapable in any event of producing anything more than very vague metaphors and perhaps even eventually contradictory results.

Shall we limit ourselves to saying that the divine personality is essentially incomparable and transcendent? Very well, but if so it is very badly named, and in a way which seems made expressly to induce delusion. For if we declare that the divine personality does not resemble in any respect that with which we are acquainted, what right have we to call it "personality"? Logically it should be designated by a word which would belong only to God, which could not be employed in any other instance. This word would therefore be intrinsically undefinable. Let us imagine any assemblage whatever of syllables deprived of all possible significance. Let A be this assemblage. Then

by our hypothesis "God is a person" does not have any other meaning than "God is A." Is this an idea?

The dilemma is unsolvable for any one who is seeking an intellectualist interpretation of the dogma "God is a person." Either he will define the word "personality," and then he is fatally sure to fall into anthropomorphism; or he will not define it, and then he will fall none the less fatally into agnosticism. Here we have a circle.

The same remarks hold with regard to the propositions "God is conscious of himself; God loves, wills, thinks, etc."

Let us take another example, the resurrection of Jesus. If this dogma, whatever may be eventually its practical consequences, has for its first aim to increase our knowledge in guaranteeing to us the accuracy of a certain fact, if it constitutes before all a statement of an intellectual character, the question to which it first gives rise is this: What precise meaning does it assume is to be attached to the word "resurrection"? Jesus, after having experienced death, has once more become alive. What does this mean from the theoretical point of view? Doubtless nothing except that after three days Jesus reappeared in a state identical with that in which he was before he was nailed on the cross. Now the Gospel itself tells us exactly the opposite. The resurrected Jesus was no longer subject to ordinary physical or physiological laws; his "glorified" body was no longer perceptible in the same conditions as before, etc. What does this mean? The idea of life has not the same content when applied to the period preceding the crucifixion as to that which followed it. Now what does the word represent with relation to this second period? Nothing that can be expressed by concepts. It is simply a metaphor which cannot be converted into specific ideas. Here again, to be exact, it would be necessary to create a new word, a word reserved for this single case, a word

consequently to which it would not be possible to give any regular definition.

Let us borrow a final example from the dogma of the real presence. Here it is the term "presence" which must be interpreted. What does it usually signify? A being is said to be present when he is perceptible, or when though he himself cannot be grasped by perception he yet manifests himself by perceptible effects. Now according to the dogma itself neither of these two circumstances is realized in the case in hand. The presence in question is a mysterious presence, ineffable, unique, without analogy to anything that one ordinarily understands by that name. Now I ask what idea is there here for us? A thing that can neither be analyzed nor even defined could not be called an "idea" except by an abuse of the word. We wish a dogma to be a statement of an intellectual order. What does it state? It is impossible to say exactly. Does not this fact condemn the hypothesis?

Finally the pretension of conceiving dogmas as statements whose first function would be to communicate certain theoretical bits of knowledge would run against impossibilities on every hand. It seems to end inevitably in reducing dogmas to pure nonsense. Perhaps it must for this reason be resolutely abandoned. Let us therefore see what different kind of significance remains possible and legitimate.

* * *

First of all, if I do not deceive myself, a dogma has a *negative* meaning. It excludes and condemns certain errors instead of positively determining the truth.[16]

Let us once more take up our former examples. We shall first consider the dogma "God is a person." I nowhere see in it any definition of the divine personality. It teaches

[16] We shall shortly see how dogmas are more and greater than this. But at the start I shall place myself in a strictly intellectualist point of view.

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me nothing about that personality. It does not reveal its nature to me nor furnish me with any explicit idea. But I see clearly that it tells me, "God is not impersonal"; that is to say, God is not simply a law, a formal category, an ideal principle, an abstract entity, any more than he is a universal substance, or some unknown cosmic force diffused throughout the world. In short, the dogma "God is a person" does not bring to me any new positive conception nor does it any more guarantee to me the truth of any particular system among those which the history of philosophy shows to have been successively proposed, but it warns me that this or that form of pantheism is false and ought to be rejected.

I would say the same with regard to the real presence. The dogma does not tell me any theory about that presence, it does not even teach me in what it consists; but it tells me very clearly that it must not be understood in such or such a way as were formerly proposed, that for instance the consecrated host must not be regarded solely as a symbol or a figure of Jesus.

The resurrection of Christ gives rise to the same remarks. This dogma does not teach me in any degree what was the mechanism of this unique fact nor of what kind the second life of Jesus was. In short it does not communicate a conception to me. But on the contrary it excludes certain conceptions that I might be tempted to make. Death has not put an end to the activity of Jesus with reference to the things of this world. He still mediates and lives among us, and not at all merely as a thinker who has disappeared and left behind a rich and living influence and whose work has left results through the ages; he is literally our contemporary. In short, death has not been for him, as it is for ordinary mortals, the definite cessation of practical activity. This is what the dogma of the resurrection teaches us.

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Shall I insist further? It does not seem advisable at this time. The foregoing examples are sufficient to make the principle of interpretation that I have in mind clearly understood. Of course long expositions would be necessary if we would enumerate in detail all the consequences of this principle and all its possible applications, and an enumerative study of the different dogmas would therefore become indispensable. But this is not my real purpose. I wish to confine myself simply to indicating an ideal. This is why I do not undertake either to multiply examples or even to develop any one of them completely.

Moreover the idea is not a new one. It belongs to the most authentic tradition. Is it not indeed the classical teaching of theologians and scholars that in supernatural matters the surest method of investigation is the *via negationis*? Permit me to recall in this connection a well-known text of St. Thomas: "But the *via remotionis* is to be used chiefly in considering divine substance. For divine substance by its immensity exceeds every form which our mind can touch; and so we cannot grasp it by knowing what it is, but some sort of a notion of it we have by knowing what it is not."[17]

Nevertheless I ought to point out one objection which might occur to the mind. We will easily grant that the dogmatic formulation promulgated by the church in the course of history has especially a negative character, at least when looked upon from an intellectual point of view as we are doing at this time. In fact, the church itself declares that its mission is not in the least to produce new revelations but only to maintain the *depositum revelationis,* and the negative method here adopted is entirely suitable for this mission. And yet, of what does this *depositum*

[17] "Est autem via remotionis utendum praecipue in consideratione divinae substantiae. Nam divina substantia omnem formam, quam intellectus noster attingit, sua immensitate excedit; et sic ipsam apprehendere non possumus cognoscendo quid est, sed aliqualem ejus habemus notitiam cognoscendo quod non est."—*Contra Gentiles,* I, xiv.

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consist if not of a certain collection of original affirmations? Take the primary expression of Christian faith, the Credo. What could be more positive? Now here is the basis of doctrine, that which characterizes and constitutes it. Moreover when we say "revelation" we certainly say affirmation and not negation.

Certainly we do. I do not contradict it in the least, but we must make a distinction. The creed of Nicaea and Constantinople contains many traces of a negative dogmatic elaboration: for instance, on the divinity of Christ as against the Arian heresy; on the procession of the Holy Ghost in opposition to the Macedonians, etc.[18] Consequently there is nothing on this head to contradict our conclusions. It is only the grammatical form which is affirmative here; in reality we are treating of errors to be excluded rather than theories to be formulated. But let us take the Apostles' Creed. Here indeed we have nothing negative but neither do we have anything properly intellectual and theoretical, nothing which belongs properly to the order of speculative knowledge, nothing in short which resembles the statement of theorems. It is a profession of faith, a declaration of attitude. We shall soon examine dogmas from this practical point of view (which I hasten to say is in my eyes the principal point of view), yet we shall stop a moment at the intellectual point of view. The Apostolic Creed in its original form affirms the existence of realities of which it gives not even a rudimentary representative theory, hence its only role with reference to abstract and reflective knowledge is *to state objects and therefore problems*. Finally we see that the proposed objection is not valid and we can maintain our thesis until further notice.

Thus in so far as they are statements of a theoretical order dogmas have all a negative meaning. History proves

[18] It would be easy to insist on the example of *consubstantialem* or of *Filioque*.

this when it procures our assistance at the birth of one after another of them in relation to the several heresies.[19] The rise of all dogmas has always followed the same course, has always presented the same phases: at the beginning purely human speculations, some explanatory systems very similar to other philosophical systems, in short, attempts at theories relating to religious facts, to mysterious realities experienced by Christendom in its practical faith; then only come the dogmas for the purpose of condemning certain of these attempts, of taxing certain of these conceptions with error and of excluding certain of these intellectual representations. Hence it follows that dogmatic formulas often borrow expressions from different philosophies without taking the trouble to fuse together and unify these heterogeneous languages.

This offers no more disadvantages than does the use of concepts derived from different origins, from the moment that dogmas do not tend to constitute by themselves a rational theory, an intelligible system of positive affirmations, but confine themselves to opposing certain exceptions to certain hypotheses and conjectures of the human mind. On the other hand it is natural that each dogma should put itself in the point of view belonging to the doctrine that it lays under an interdict, in order to attack it directly without danger of ambiguity. Hence it also follows that dogmatic formulas can enact laws on the incomparable and the transcendent and yet not fall into the contradictions of anthropomorphism or of agnosticism. It is man who with his opinions, his theories and systems, gives to dogmas their intelligible substance;[20] these are confined to pronouncing a veto at times, to declaring at times that

[19] Compare the usual formula of the decrees of council: "*Si quis dixerit...*, *anathema sit.*"

[20] From the theoretical point of view, understand. Dogmas are thought in terms of the human systems which they oppose. [This view is recently endorsed by Catholic theologians of such recognized authority as Cardinal Billot, S. J.—Tr.]

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"such an opinion, such a theory, such a system, is not allowed," without ever pointing out why they should not be accepted, nor by what they must be replaced. Thus negative dogmatic definitions do not limit knowledge nor put an end to progress; in short they only close up false paths.

From the strictly intellectual point of view it seeems to me that dogmas have only the negative and prohibitive sense of which I speak. If they formulated absolute truth in adequate terms (to assume that such a fiction has a meaning) they would be unintelligible to us. If they gave only an imperfect truth, relative and mutable, they would not be justified in obtruding themselves. The only radical way to put an end to all the objections on principle against dogma is to conceive of them, as we have already said, as being undefinable in so far as they are speculative propositions, except with relation to previous doctrines upon which they promulgate an unwarranted judgment. Moreover is it not the teaching of theologians, including the most intellectualist, that in a dogmatic statement the reasons which can be incorporated in the text are not in themselves objects of faith imposed upon belief?

There is one important consequence resulting from the foregoing, namely, that the true method of studying dogmas (from the intellectual point of view, understand) is the historical method. The science known as positive theology, or rather the history of dogma, seeks to perform this task. The method has an effective apologetic value much greater than purely dialectic dissertations. Because in any event it is impossible to comprehend dogmatic statements, there is the greater reason for justifying them if one would commence by plunging them once more into their natural historical environment without which their authentic meaning becomes more and more vague and finally ends by vanishing entirely.

Nevertheless dogmas do not have merely a negative

meaning, and even the negative meaning that they offer when regarded from a certain direction does not constitute their essential and primary significance. This is true because they are not merely propositions of a theoretical character, because they must not be examined solely from the intellectual point of view, from the point of view of knowledge. This is what we shall now elucidate further.

* * *

Here more than ever I insist that the intention and tendency of the pages to follow must not be misunderstood. I repeat that the affirmative tone is used only as a means for clearness. At bottom the question is always the same as I specified at the beginning. Here, if I may say so, is the form in which experience has shown me that the notion of dogma is most easily assimilable to the minds of to-day:

A dogma has above all a *practical* meaning. It states before all a prescription of a practical kind. It is more than all the formula of a rule of practical conduct. This is its principal value, this its positive significance. This does not mean, however, that it must be without relation to thought, for (1) there are also certain duties concerned with the act of thought; (2) it is virtually affirmed by the dogma itself that under one form or another reality contains wherewith to justify the prescribed conduct as reasonable and wholesome.

I take pleasure in quoting in this connection the following passage from R. P. Laberthonnière:[21] "Dogmas are not simply enigmatical and obscure formulas which God has promulgated in the name of his omnipotence to mortify the pride of our spirits. They have a moral and practical meaning; they have a vital meaning more or less accessible to us according to the degree of spirituality we possess."

After all, when converts, in spite of good intentions,

[21] *Essais de philosophie religieuse*, p. 272. Paris: Lethielleux.

themselves create part of the theoretical difficulties under discussion do we not answer them daily: "Never mind all that, it is not important. Do not believe that God requires so many formalities. Come to him fairly, frankly, simply, according to the wise words of Bossuet. Religion is not so much an intellectual adherence to a system of speculative propositions as it is a living participation in mysterious realities." Why not then make theory agree with practice?

Let us keep the same examples. They represent well enough the different types of dogmas. "God is a person" means, "Conduct yourself in your relations to God as in your relations with a human person." Likewise "Jesus has risen" means, "Be in relation to him as you would have been before his death, as you are with a contemporary." In the same way again the dogma of the real presence means that one must have the same attitude toward the consecrated host as one would have toward Jesus had he become visible, and so on. It would be easy to multiply these examples, and also to develop each of them farther.[22]

That dogmas can and ought to be interpreted in this way there is no doubt, and the fact will not be contested by any one. In fact, it cannot be repeated too often that Christianity is not a system of speculative philosophy but a source and regimen of life, a discipline of moral and religious action, in short the sum total of practical means to obtain salvation. What then is surprising in the fact that its dogmas primarily concern conduct rather than pure reflective knowledge?[23]

I do not think it is necessary to insist farther upon this

[22] I do not claim in the least that the foregoing comments exhaust the meaning of the dogmas mentioned: they will suffice to point out a line of inquiry.

[23] This is why assent to dogmas is always a free act and not the inevitable result of a compelling line of argument.

point, but I wish to indicate in a few brief words the most important consequences of the principle here laid down.

First of all it is clear that the general objections summed up at the beginning of this article do not affect this conception of dogma to the same extent and in the same degree as they do the usual intellectualist conception, for that provokes the conflict and renders the difficulty insurmountable, whereas on the other hand we may now catch a glimpse of a possible solution. As there is no question of obtaining a theoretical statement in conditions radically opposed to those prescribed by scientific method, we no longer find ourselves face to face with a logical stumbling-block but only with a problem referring to relations between thought and action—a difficult problem certainly, but not unapproachable and one which at any rate does not appear absurd after it is stated.

Of course there are always important questions to be solved. It is necessary to supply the dogma in some way with a demonstration and justification, and this is by no means a perfectly easy matter. Nevertheless one of the greatest obstacles has been smoothed away. Practical truths are established differently from speculative truths. Recourse to authority which is entirely inadmissible in the realm of pure thought seems *a priori* less shocking in the domain of action, because if authority has legitimate rights anywhere it certainly has in the domain of practical affairs.

The Council of the Vatican tells us: "If any shall say that no true mysteries properly so-called are contained in divine revelation, but that all the dogmas of faith can be comprehended and demonstrated through reason duly perfected by natural principles, let him be anathema."[24] Now if faith in dogmas were first of all knowledge, an adherence to some statements of an intellectual kind, one

[24] "Si quis dixerit in revelatione divina nulla vera et proprie dicta mysteria contineri, sed universa fidei dogmata posse per rationem rite excultam a naturalibus principiis intelligi et demonstrari, anathema sit."

could not comprehend either that assent to unsolvable mysteries could ever be legitimate or even simply possible, or in what it might consist, or what sort of utility or value it might have for us, or how it might constitute a virtue. On the other hand all this can be understood if faith in dogmas is a practical submission to commandments which have to do with action. Nothing is more normal than activity placing mysteries before intelligence.[25]

The Council of the Vatican tells us further: "If any shall say that assent to the Christian faith is not free.... let him be anathema."[26] This text is generally explained by recognizing that the reasons for believing, the motives of credibility, are not of insuperable force, a mathematical evidence, and that in consequence a decisive act of the will or of the heart is always necessary to conclude the investigation definitely. Is this not virtually admitting that one cannot see in belief in dogmas an act which should first of all be intellectual without making it thereby inferior to the ordinary acts of thought? How would such an act—an act performed under conditions contrary to the nature of thought—be even legitimate or merely possible? But on the other hand it is easy to believe that the practical acceptance of commandments relating to action depends on our free will and gains in perfection by not being able to manifest itself by necessary consequence. Let us insist a little upon this point, for it is of highest importance in the problem of the relations between reason and faith.

From the beginning apologetics is confronted with a grave difficulty which perhaps cannot always be satisfactorily disposed of. On the one hand it is clearly understood that an act of faith is a free act and that its object, as well as its supreme motive, is supernatural. But on the other

[25] Submission to dogmas then from one point of view is for the believer what submission to facts is for the scholar.

[26] *"Si quis dixerit assensum fidei Christianae non esse liberum...., anathema sit."*

hand an act of reason ought to precede and prepare the act of faith, for it is reason alone by which the obligation and necessity of overreaching reason can be recognized. And an act of reason must also constantly accompany the act of faith, for it is necessary that the human mind shall have some sort of hold upon the dogma if it wishes to accept it. St. Thomas said well: "Those things which are under the faith....no one would believe unless he sees they ought to be believed."[27]

Now how shall we reconcile these two opposite requirements in a system of intellectualist interpretation? Either we would maintain (as there are some who do) that the apologetic proofs are absolutely positive and exact; and then what would become of the liberty of the act of faith? Or in order to safeguard that liberty we would call them insufficient and only more or less probable; and then our faith would lack any basis, for after all an insufficient proof is not an acceptable proof, especially in so important and difficult a matter. An intellectualist attitude becomes disarmed in the face of this dilemma since liberty does not belong to the domain of pure intelligence and has no place or part in the proceedings of discursive reason. But with the other attitude the dilemma can be solved because this time the dialectic in the case is action and life not simply argument, and liberty revives with life and action.

Likewise we have here the objection relating to the intelligibility of dogmatic formulas. Although these formulas are hopelessly obscure, even inconceivable, when we want them to furnish positive determinations of truth from a speculative and theoretical point of view, they nevertheless show themselves capable of clearness if we are careful not to ask of them anything but instruction as to practical conduct. What difficulty, for instance, do we find in understanding the dogmas of the divine personality, of the real

[27] "Ea quae subsunt fidei.... nemo crederet nisi videret ea esse credenda."

presence, or the resurrection in the practical system of interpretation just outlined? Although these dogmas are mysteries for the intelligence that demands explanatory theories they are nevertheless susceptible of perfectly clear statement as to what they prescribe for our actions. Hence the language of common sense has its place as well as the use of anthropomorphic symbols and the employment of analogies or metaphors, and neither the one nor the other gives rise to unsolvable complications since this time it is a question only of propositions relating to man and his attitudes.

We also see now what the relation is between dogmas and efficient life. We predict for them a possibility of experimental study and of gradual research which has heretofore escaped us. Finally we understand how they can be common to all, accessible to all, in spite of the inequality between intellects, whereas to conceive them in the intellectualist way one would be inevitably led to make a distinction of an intellectual aristocracy. I have not room here to develop these different considerations as much as I should like, but I imagine that a simple indication after all may be sufficient for the time being, and that the reader can carry the process on for himself without any difficulty. Nevertheless it seems necessary to me to prevent a possible objection in order to avoid all misapprehension.

I have spoken of *practice*. This word must be rightly understood. I take it in the widest acceptation of the term. *Action* and *life* are here synonymous. Hence the word does not in the least mean a blind step, without relation to thought or consciousness. In fact there is an act of thought which accompanies all our actions, a life of thought which mingles throughout our life; in other words, to know is a function of life, a practical act in its way. This function, this act, is also called *experience,* a name which indicates at the same time that we are not at all

dealing with actions performed without any sort of light but that the light in question is not that of simple argumentative reason.

I have also spoken of the activity which places mysteries before intelligence, and by way of elucidation I have cited the example of scientific facts. To comprehend what I mean by this, one must not forget that a scientific fact is not a thing to be submitted to passively. If there is any semblance of a purely external fact, of a mystery totally opaque, of a violent commandment from without, it is so with respect to argumentative understanding. But the thought-action of which I was just now speaking avoids this appearance. It infinitely exceeds the purely intellectual thought. I have not heard anything to affirm otherwise.[28]

Hence there is a necessary relation between dogmas and thought. It is at the same time both a right and a duty not to be content with a blind belief in dogmas but to strive also in proportion to one's strength to think them. The system of separation, of tight partitions, of the twofold accountability of conscience, is not desirable nor, to speak truthfully, possible. It is contrary to the demands of that faith which wishes to hold every man; it is contrary to the requirements of philosophy which desires a spiritual unity; and finally it is contrary to the requirements of morality which cannot approve an action that is systematically unconsidered.

But thought when applied to dogmas should not misunderstand their primarily practical meaning. The path to be followed is the test of practical experience and not an intellectual dialectic. The inspiring principle is perfectly expressed in the sacred word, *qui facit veritatem venit ad lucem.*

[28] The reader who desires to pursue this point further may refer to several articles I have published since 1889 in the *Revue de métaphysique et de morale,* and in the *Bulletin de la société française de philosophie.*

Thus translated into terms of action the traditional methods of *analogy* and *eminence* assume a very clear significance. Under the guise of metaphors and images they affirm that supernatural reality contains the wherewith to make obligatory by law that our attitude and our conduct with regard to it should have such or such a character. The images and metaphors—which are hopelessly vague and fallacious when one tries to see in them any approximation whatever of impossible concepts—become on the other hand wonderfully illuminating and suggestive after one looks to find in them only a language of action translating truth by its practical echo within ourselves.

It remains finally to specify the relations of dogmas, understood in the way we have described them, to theoretical and speculative thought, to pure knowledge. In what respect do they govern our intellectual life? How does their intangible and transcendent character leave the full liberty of research intact as well as the undeniable right of the mind to repulse every conception which tries to impose itself from without? We shall easily see.

The Catholic is obliged to assent to the dogmas without reservation. But what is thereby imposed upon him is not in the least a theory, an intellectual representation. Such a constraint indeed would inevitably lead to undesirable consequences: (1) The dogmas would in that case be reduced to purely verbal formulas, to simple words whose repetition would constitute a sort of unintelligible command; (2) Moreover these dogmas could not be common to all times nor to all intelligences.[29]

No, dogmas are not at all like that. As we have seen, their meaning is above all practical and moral. The Catholic, obliged to accept them, is not restrained by them except as regards rules of conduct, not as regards any par-

[29] In the two words "esotericism" and "Pharisaism" would be the inevitable double rock upon which they would split.

ticular conceptions. Nor is he condemned to accept them as simple literal formulas. On the contrary, they offer him a very positive content, explicitly intelligible and comprehensible. I will add that this content, having to do solely with the practical, is not relative to the variable degree of intelligence and knowledge; it remains exactly the same for the scholar and the ignorant man, for the exalted and the lowly, for the ages of advanced civilization and for the races still in barbarism. In short it is independent of the successive states through which human thought passes in its effort toward knowledge, and thus there is *only one faith for everybody.*

This being granted, the Catholic after having accepted the dogmas retains full liberty to make for himself whatever theory, or whatever intellectual representation he wishes of the corresponding objects—the divine personality, the real presence, or the resurrection, for instance. It remains with him to grant his preference to the theory which best agrees with his own views, to the intellectual representation which he deems the best. His position in this respect is the same as that toward any scientific or philosophical speculation, and he is free to adopt the same attitude in both cases. Only one thing is imposed upon him, only one obligation is incumbent upon him; his theory must justify the practical rules expressed by the dogma, his intellectual representation must take into account the practical edicts prescribed by the dogma. Thus in a word it appears almost like the statement of a fact with regard to which it is possible to construct many different theories but which every theory must take into account, like the expression of a truth many of whose intellectual representations are legitimate but of which no explanatory system can well be independent.[30]

[30] It is at this point that we must distinguish between *intellectual formula* and the *underlying reality* in the dogma.

From this naturally follows the step that we have recognized as usual with religious thought in its effort at elaboration. Let us take any dogma whatever, Divine Personality, the real presence or the resurrection of Jesus. By itself and in itself it has only a practical meaning. *But there is a mysterious reality corresponding to it and therefore it presents to the intelligence a theoretical problem.* The human intellect at once takes possession of this problem; and obeying simply and solely the laws of its own nature it imagines the explanations, the answers, the systems codified in the precepts of scientific method and the principles of reason.[31] As long as the theory constructed in this way respects the practical significance of the dogma it is given *carte blanche.* Hence to pass judgment on the theories remains the task of pure human speculation, and any authority exterior to the thought itself has neither the right nor the power to interfere.[32] But once let a theory arise which makes an attack on dogma in its own domain by altering its practical significance, and the dogma would immediately array itself against it and condemn it, thus becoming a negative intellectual statement superimposed upon the rule of conduct which at first it was, purely and simply.

Hence one sees positively how the two meanings of a dogma, the practical meaning and the negative meaning, are reunited, the latter being subordinated to the former. Moreover we see how dogmas are immutable and yet how there is an evolution of dogmas. What remains constant in the dogma is the orientation that it gives to our practical activity, the direction in which it inflects our conduct. But

[31] In this respect the Middle Ages had an independence and a boldness which we have forgotten.

[32] Religious authority which has souls in its charge can indicate certain theories as dangerous, as long as they run the risk of being wrongly understood and thus of reacting injuriously upon conduct. Hence arise censures of an inferior note to those of heresy. But these condemnations are not properly dogmatic.

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the explanatory theories, the intellectual representations, change constantly in the course of the ages according to individuals and epochs, freed from all the fluctuations and all the aspects of relativity manifested by the history of the human mind. The Christians of the first centuries did not profess the same opinions on the nature and personality of Jesus as we, and they did not have the same problems. The ignorant man to-day does not have at all the same ideas on these lofty and difficult subjects as the philosopher does, nor the same mental preoccupations. But whether ignorant men or philosophers, men of the first or the twentieth century, every Catholic has always had and always will have the same practical attitude with regard to Jesus.

* * *

It is time to conclude and I will do so in as few and brief words as possible.

Two main results seem to me to have been attained by the foregoing discussion:

1. The intellectualist conception which is current to-day renders the greater number of objections raised by the idea of dogma unsolvable.

2. On the other hand, a doctrine of primacy of action permits a solution of the problem without abandoning either the rights of thought or the requirements of dogma.

If these conclusions were admitted, the apologetics of our days would be under the irresistible necessity of modifying many of its arguments and methods.

Now, can these conclusions be admitted without loss to faith? It is for the theologians to tell us, and in case their response is negative to teach us how they expect otherwise to prepare to surmount the obstacles which perplex us.

EDOUARD LE ROY.

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LEIBNIZ IN LONDON.[1]

LEIBNIZ paid two visits to London from Paris, where he was staying from March, 1672, to October, 1676: the first visit, which was in connection with the embassy from the Elector of Mainz, was from January 11 to the beginning of March, 1673; the second was made on his way home to Germany, when he stopped in London for about a week in October, 1676.

Leibniz had a habit of writing out all the important scientific points in the correspondence that he kept up with noted people, so that he might thus impress them the more deeply upon his memory. I have discovered among his manuscripts three folio sheets on which he has written down the things worth noting in connection with these two visits to London.[2] The sheets which relate to his second visit have been known to me for some time; but the other ones, referring to the first visit, I came across only during my last stay in Hanover in the summer vacation of the year 1890.

In what follows, I have only paid attention to the contents of these sheets which refer to mathematics.[3]

[1] Translated by J. M. Child, from an article by Dr. Gerhardt in the *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*, 1891, pp. 157-165. The notes are by the translator.

[2] These highly important documents ought to be photographed and published in facsimile.

[3] It seems a pity that Gerhardt has not given the contents of the section labeled "Mechanica," unless indeed this is all non-mathematical; there may

The sheet relating to Leibniz's first visit to London, of which I have added a partial transcript under the heading I, is divided on both pages into sections [the word used in the original is *Felder* = columns, but it will be seen that, according to the transcript given later, the sections are horizontal and not vertical], in which Leibniz has entered all that he considered to be worth noting. While the sections labeled "Chymica," "Mechanica," "Magnetica," "Botanica," "Anatomica," "Medica," and "Miscellanea" are filled up with an extraordinary number of memoranda, the first sections, which are allotted to mathematical subjects, are very poorly filled. That labeled "Geometrica" contains a note that is especially worth remarking: "Tangents to figures of all kinds. Development of geometrical figures by the motion of a point in a moving straight line."[4] In all probability it may be supposed that this refers to the lectures of Barrow, delivered on his method of tangents at the University of Cambridge down to the year 1669. As is well known, the method of Barrow is only applicable to such curves as can be expressed by rational functions.[5] Newton's name was mentioned in

be in it some intimation that would lead to a clue as to the origin of Leibniz's use of the word *moment*, meaning thereby, not Newton's use of the word, but the idea now familiar to us in the determination of the center of gravity of an area, expressed by the equation

$$x = \Sigma ax/\Sigma a,$$

where a is the element of the area distant x from the axis, x the distance of the center of gravity from that axis, and Σax is the sum of the 'first moments of the elements' or 'the first moment of the whole area.' See note 16, later.

[4] "*Tangentes omnium figurarum. Figurarum geometricarum explicatio per motum puncti in moto lati.*"

[5] In a footnote, Gerhardt asserts that "Barrow's *Lectiones Geometricae* appeared in 1672." This is incorrect; for they were published, combined with the second edition of the *Lectiones Opticæ*, in 1670; nor can Gerhardt be referring to the second edition, for that appeared in 1674 and then as a separate volume. Also, I have, in the little book on *The Geometrical Lectures of Isaac Barrow*, published by the Open Court Publishing Co., given reasons for supposing that these lectures were never delivered as *Lucasian Lectures*, though they may have formed the subject-matter for college lectures at Gresham and Trinity. Again, it is not true, although "well known," that "the method of Barrow was only applicable to such curves as can be expressed by rational functions"; this remark is even only partially true about the differential triangle method; for, as I have shown in the above-mentioned book, Barrow had a complete calculus, which included, among other things, the important idea of *substitu-*

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the "Optica." Leibniz has the remark: "They told me about a certain phenomenon that Barrow confessed he was unable to solve. Newton's difficulty has so far not been solved, Father Pardies having given it up."[6] Obviously this remark applies to Newton's experiment on the refraction of light by a prism and to the decomposition of white sunlight, and especially to the fact that a circular solar image becomes after refraction a long spectrum. Father Pardies of Clermont had published in opposition to Newton his "Two Letters containing Animadversions upon I. Newton's Theory of Light," in the *Philosophical Transactions* of 1672, together with a letter from Newton.

It cannot be said for certain that Leibniz, during his first stay in London, met with any of the great English mathematicians; Wallis lived at Oxford, while Barrow and Newton resided at Cambridge.[7] Indeed, it is made a matter of plaint by Brewster, the biographer of Newton, that the Royal Society of London at that time numbered few men of distinguished talents who were in a position to perceive the truth of the optical discoveries of Newton. In the letter which Leibniz addressed to Oldenburg, the Secretary of the Royal Society, during his visit to London, he men-

tion, which is all that is necessary to complete the "*a*-and-*e*" method and make it applicable to surds and fractions, and probably was thus applied by Barrow in working out his constructions; but the whole thing was geometrical, which apparently hid the inner meaning until recently.

To my mind, the mention of but "tangents and local motion" points out that, on Leibniz's *first* reading of Barrow, he only perused at all carefully the first five lectures, which are relatively unimportant; or rather it confirms an opinion I had already expressed to Mr. P. E. B. Jourdain.

[6] "*Locuti sunt mihi de phaenomeno quodam quod Barrovius fatetur se solvere non posse. Newtoni difficultas soluta hactenus non est, P. Pardies manus dante.*"

[7] It seems however that Leibniz attended the meetings of the Royal Society; at any rate once, when he exhibited the model of his calculating machine. It would be interesting if the roll of members present on all occasions during this period could be obtained, as doubtless they were kept. For such men as Ward were members at the time and attended the meetings, and Ward was, if not in the same class as the three whose names are given, an excellent mathematician; and, Leibniz, being somewhat of a notable, on account of his connection with the Embassy from Mainz, would surely be introduced to all eminent members present.

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tioned that he had met by accident the mathematician Pell at the house of Boyle, the chemist. The conversation fell upon those number-series which in elementary mathematics were called the higher arithmetical series and whose sums and terms were found by the help of differences. Leibniz showed that he had gone deeply into the study of such series and had partly found out some new methods for calculating the terms.[8] Leibniz's letter to Oldenburg was dated Feb. 3, 1673 (1672 O. S.).[9]

From the preceding it appears that what Leibniz learned with reference to mathematics from his first visit to London was quite unimportant.[10] The chief aim of his stay in London was to be elected as a Fellow of the Royal Society; and this came to pass, owing in part to an exhibition of a model of his calculating machine, and in part to the friendly offices of Oldenburg.

After his return to Paris at the beginning of March, 1673, Leibniz was able to find more leisure to follow up his studies without hindrance; the political mission which was the cause of his being sent to Paris, was now at an end.

It may be regarded as certain that, before his first visit to London, Leibniz made the personal acquaintance of the men with whom he corresponded before he came to Paris, and especially Antoine Arnauld and de Carcavy. The

[8] The account given by Leibniz himself in the *Historia* (see *The Monist* for October, 1916) reads thus: "He" [for Leibniz wrote in the third person, under the guise of "a friend who knew all about the matter"] "also came across Pell accidentally, and described to him certain of his own observations on numbers, and the latter stated that they were not new, but it had been recently made known by Nikolaus Mercator.... This made Leibniz get the work of Nikolaus Mercator." As a matter of fact the suggested plagiarism, or what Leibniz took for such a suggestion, was from Mouton and not from Mercator. This is an instance of the lack of memory from which Leibniz suffered; such lack as caused him to make notes of all important points.

[9] See Note 32, on the introduction of the Gregorian calendar.

[10] I cannot see what reason Gerhardt has for this statement, considering the contents of Barrow's book, which we know that Leibniz had purchased; that is, unless we assume either that Leibniz, as I have suggested, did not at that time read the whole of Barrow, or failed to grasp what Barrow had given owing to his (Leibniz's) incomplete knowledge of geometry.

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latter belonged to the circle in which Pascal moved. Whether at that time Leibniz had made the acquaintance of Huygens is not quite so certain; at any rate he did not come into close relations with him until after his return from London. Huygens presented him with a copy of his great work, *Horologium Oscillatorium,* which had just (1673) been published. The recognition that his mathematical knowledge at that time was insufficient to enable him to understand the contents of this book, combined with a reawakening of his former love for mathematics, had the effect of making Leibniz devote himself with the greatest fervor to the study of mathematcial subjects. Cavalieri's method of indivisible magnitudes, the writings of Gregory St. Vincent, the letters of Pascal (which were especially recommended to him by Huygens), were used by him as guides in his studies. As the first-fruits of these studies, he obtained the theorem that, when the square on the diameter of a circle was taken as unity, the area of the circle was expressed by the infinite series

$$1-\frac{1}{3}+\frac{1}{5}-\frac{1}{7}+ \ldots\ldots \text{ ad inf.} \quad (11)$$

He obtained it thus: Instead of dividing the circle, as in the method of Cavalieri, into trapezia by means of parallels, he divided it into triangles by lines radiating from a point; the areas of these triangles being proportional to certain lines. With these lines as perpendicular ordinates a curve could be constructed that was divided by these ordinates into trapezia, each of which is double the corresponding triangle. In this way Leibniz obtained a curvilinear figure[12] whose area was double that of the circle, but which was expressed by a rational function, $x = y^2/(1 + y^2)$,[13]

[11] Leibniz's own date for the discovery of this result, usually alluded to by him as the "Arithmetical Tetragonism," is 1674; "But in the year 1674 (so much it is possible to state definitely) he came upon the well-known Arithmetical Tetragonism;...." (See *Historia,* in *The Monist,* Oct., 1916.

[12] See the first critical note, page 536.

[13] See the first critical note, page 536.

of its coordinates; and, using a method that was similar to that employed by Mercator for the equilateral hyperbola, this area could be found (Quadratrix).[14]

For the rest of Leibniz's treatment, see the hitherto unpublished manuscript, given under II in the appendix that follows.

As was often the case in the first scientific studies of Leibniz, intimations of the great problems that occupied his attention his whole life through are found here in his first efforts in the domain of higher mathematics. First is it to be remarked that Leibniz abandoned the division of curvilinear figures into trapezia, as was done by Cavalieri, and instead divided them into triangles; from this he was led to the "characteristic triangle,"[15] which formed the foundation in the application of the differential calculus. Further, Leibniz constructed, instead of the proposed curve, another of which the area could be found (the "quadratrix" as he called it); this method of procedure frequently occurs in the later works of Leibniz on the integral calculus. Closely connected also with this is the solution of the inverse method of tangents, that is, given the tangent, to find the curve.

In these first efforts of Leibniz in the domain of higher mathematics is clearly to be seen the influence of his study of the writings of Pascal.[16] The French mathematicians Roberval and Pascal did not consider that Cavalieri's

[14] Observe that Leibniz (or Gerhardt) employs this word in a different sense from that of Barrow, with whom it means the special curve whose equation is $y = (r - x)\tan \pi x/2r$, a curve that is particularly connected with the circle.

[15] This contradicts both Gerhardt and Leibniz himself, who said that he got it from a consideration of a figure used by Pascal in finding the content of the sphere. See also the critical note referred to in 12, 13 above.

[16] I hope to consider this influence in a later number of *The Monist*, in connection with an essay by Gerhardt on this very point; when I shall endeavor to substantiate an opinion I have formed with regard to the earlier manuscripts of Leibniz, which were discovered by Gerhardt, and of which translations appear in *The Monist* (April, 1917). I suggest that these do not represent so much the record of his original investigations as notes made while using the works of his predecessors as text-books.

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method was consistent with the rigorous requirements of mathematics;[17] they reverted to the study of the Greek mathematicians, and especially to the writings of Archimedes, combining with their method the developments which Kepler, in particular, had brought about by the introduction of infinitely small magnitudes into geometry. Moreover, in connection with Pascal, it is to be observed that he generalized into a "barycentric calculus" the procedure used by Archimedes for the quadrature of the parabola by means of the equilibrium of the lever.[18] This "calculus" enabled him to solve problems on the cycloid which his contemporaries had vainly attempted.[19] It was not unknown to Leibniz that, since the time of Pappus of Alexandria, quadratures and cubatures had been calculated by the aid of the center of gravity (Guldin's rule, "Centrobaryca"); certainly he was now led, by the works of Pascal, again to notice the methods for the determination of the center of gravity, and was also induced to attempt to extend and perfect them. The manuscript of

[17] I fail to see how this statement can be completely reconciled with the following well-known quotation from the *"Lettre de A. Dettonville à Carcavy"* (1658):

"J'ay voulu faire cét advertissement pour monstrer que tout ce qui est demonstré par les veritables regles des indivisibles se demonstrera aussi à la rigueur et à la maniere des anciens; et qu'ainsi l'une de ces Methodes ne differe de l'autre qu'en la maniere de parler; ce qui ne peut blesser les personnes raissonnables quand on les a une fois avertyes de ce qu'on entend par là" (Vol. VIII, p. 352).

Pascal also says on p. 350: *".... la doctrine des indivisibles, laquelle ne peut estre rejettée par ceux qui pretendent avoir rang entre les Geometres."*

That is, the method of indivisibles does not differ from the method of exhaustions, except in the way the argument is put; and that the former must be accepted by any mathematician with pretensions to rank among geometers.

The page reference is to the edition of Pascal's Works in 14 volumes, in the series, *Les Grands Ecrivains de la France* (pub. Hachette et Cie., Paris, 1914).

[18] Pascal calls it "la balance." It is worth noting in this connection that Pascal uses the word *"force"* and not *"moment"* for the product of one of his weights and its lever-arm; so that we must look elsewhere for the clue to the use of the word "moment" in this sense by Leibniz.

[19] Several of the problems proposed were solved by Huygens, de Sluze, and Wren; but by special methods, which did not satisfy Pascal, who called for a general method. Later (1670) Barrow gives the rectification of the arc, as a special case of a general theorem (Lect. XII, App. 3, Ex. 2, see my *Barrow*, p. 177).

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Leibniz which is dated October 25, October 26, October 29, November 1, 1675, and which contains the investigation on the center of gravity, is headed, *"Analysis Tetragonistica ex Centrobarycis."*[20]

It is worth remarking that in this Leibniz continues the method by which he had found the series for the area of the circle. Incidentally these studies were the first occasion for the introduction of the symbol for a sum, i. e., the integral sign (October 29, 1675); from this as the antithesis, the sign for the difference, i. e., the symbol for differentiation, resulted.[21] The equation in which Leibniz first introduced the sign of integration was, in the notation of that time:

$$\frac{\overline{\text{omn. } l}\,\boxed{2}}{2} \sqcap \overline{\overline{\text{omn. omn. } \frac{l}{a}}}$$

that is,

$$\frac{(\text{omn. } l)^2}{2} = \overline{\overline{\text{omn. omn. } \frac{l}{a}}};$$

for which Leibniz writes

$$\frac{\int \overline{l}^2}{2} \sqcap \int \overline{\int \overline{l}.\frac{l}{a}}$$

that is, when $l = dy$,

$$\frac{y^2}{2} = \frac{1}{a}\int dy \int dy$$

After his return to Paris in March, 1673, Leibniz was in constant communication with Oldenburg, the Secretary of the Royal Society; the subjects being almost entirely mathematical. In this way he obtained his knowledge of the work of the English mathematicians. Oldenburg's mentor on all mathematical questions was John Collins, who possessed a very wide acquaintance among English

[20] A translation is given in *The Monist*, April, 1917.

[21] See the second critical note, page 543.

mathematicians; and it was through him that what they had done was communicated. In this respect special mention is to be made of the letter from Oldenburg to Leibniz, dated July 26, 1676, in which Collins informed him of a collection of letters from English mathematicians that he had in his possession. Collins mentions in it particularly that script of Newton, of December 10, 1672, in which the latter makes a communication about his method for tangents to curves, which are given by an explicit algebraical equation; he remarks that the method is only a corollary to a general procedure for solving other problems, such as those relating to rectification, determination of centers of gravity and so on.[22] Collins stated in addition that, besides what this letter showed, nothing further was known at that time about Newton's method. It was on account of these communications, and probably also on account of a letter from Newton to Oldenburg, of which Oldenburg sent a copy to Leibniz at Paris, that Leibniz was moved to make his return journey to Germany in October, 1676, by way of London. Leibniz stayed there about a week; he made the acquaintance of Collins, who willingly let him have access to his collection of treatises and letters.[23] What Leibniz found in them that he thought worth noting he set down

[22] Leibniz, in the *Acta Eruditorum* for the year 1700, says, "I can affirm that, when in 1684 I published the elements of my Calculus, I did not know any thing more of Mr. Newton's inventions in this kind, than what he formerly signified to me by his letters, viz., that he could find tangents without taking away surds;...." As Newton says in the article in *Phil. Trans.*, Vol. XXIX, No. 342, Anno 1714 (usually called the "*Recensio*") this "is very extraordinary, and wants an explanation."

[23] This is feasible, but there is another alternative given by Dr. H. Sloman (*The Claim of Leibniz to the Invention of the Differential Calculus, English edition*, pub. Macmillan, 1860), which strikes me as even more probable. Sloman's points are as follows: (1) It is highly probable that Leibniz's week in London was the *last week of that month.* (2) Oldenburg had then in his possession two letters from Newton for Leibniz, dated Oct. 24 and 26; these he showed to Leibniz. (3) As Newton himself mentions, these were blotted and hastily written; and thus Leibniz asks, on this account, that Oldenburg should let him see the tract of Newton to which they refer; which tract Leibniz knew was in the possession of Oldenburg, that is, a copy of it. For the details of the argument, occupying ten quarto pages, see the above-mentioned book by Sloman, pp. 97-106.

on two folios; the one has the heading, "*Excerpta ex tractatu Newtoni de Analysi per aequationes numero terminorum infinitas.*" This is the paper which Newton sent in June, 1699, to Barrow, from whom Collins received it on July 30, 1699. Collins made a copy of it, and sent the original back; and the original was printed in the year 1711. The other sheet has the heading, "*Excerpta ex Commercio Epistolico inter Collinium at Gregorium.*" A partial transcript of both these sheets follows under the heading III.

With regard to the extracts from Newton's paper, it is to be remarked that Leibniz was interested in the treatment of algebraical expressions of powers and in the turning of irrational expressions into the form of series by means of division and root-extraction. He noted indeed many examples in their entirety. How to get to quadratures was known to him; he merely indicated the process by the sign of a sum, i. e., by the symbol of integration. On the other hand, the part on the numerical solution of adfected equations was new to him, and this he copied out well-nigh word for word; this is the well-known Newtonian method of solution of equations by approximations. Leibniz passes over as well known to him the remark, made by Newton at the close of the quadratures, that the problems of rectification, determination of the content of solids, determination of the centers of gravity, can be solved in the same way, and also the general indication of the process to be followed in such cases. Then follows the solution of inverse problems, for instance, to find from the area the base, that is the axis of the curve. This Leibniz copied out word for word. In the same way Leibniz has extracted the conclusion of Newton's paper, "*Demonstratio resolutionis aequationum affectarum.*" At the end of his manuscript Leibniz adds: "I extracted this from the letter of

Newton, August 20, 1672, addressed to Newton."[24] Probably this means that from the letters referring to Newton, Leibniz picked out the letter dated August 20, 1672, addressed to Newton.[25] So far as the script can be deciphered,[26] its contents were a graphic representation of Newton's method of solution of equations by approximations by means of Gunter's scale. Gunter's line had been noted by Leibniz on his first visit to London.

Of quite special interest to Leibniz were the letters of mathematicians which Collins had collected; on a second folio he made excerpts from letters from James Gregory. In two letters from Gregory (1670) was Isaac Barrow extolled as the greatest, not only among living writers, but also among all those that had written before him (Barrow). Further Leibniz found among these letters the letter mentioned above of Newton to Collins of December 10, 1672;[27] he extracted what Newton had mentioned with regard to his method of finding the expression for the tangent to a curve. Leibniz added at the end of this extract, "This method differs from that of Hudde as well as from that of Sluse, in that irrationals need not be eliminated."[28]

[24] The Latin, "*Excerpsi ex Epist. Neutoni 20 Aug. 1672 ad Neuton*," as given by Gerhardt, seems somewhat unintelligible; especially the word *Neuton*. What Collins had (or what Oldenburg, as suggested by Sloman, had) was a copy of a manuscript that Newton had sent to Barrow. Gerhardt says, "so far as the script can be deciphered"; perhaps the word *Neuton* is an error of transcription, or maybe an error on the part of Liebniz, due to the juxtaposition of the *Neutoni* which comes just before. In any case, note 25 applies.

[25] I do not think Gerhardt's translation of the word *excerpsi* is correct.

[26] Gerhardt does not state whether the extract is badly written (this would show that it had been done in a very great hurry, for Sloman says that Leibniz, in his matter for publication, wrote a beautiful hand), or whether spoilt by age; in the latter case, as old-time inks contained salts of iron, the manuscript might be restored by photography, by means of a special plate, that I understand is sometimes used for detecting forgeries in deeds and notes.

[27] The letter was sent to Barrow to be sent on to Collins, probably with the object of being communicated through the latter to others; Collins seems to have been the regular channel of communication at this period, in a similar way to Mersenne.

[28] So we find in a manuscript, dated July 11, 1677, first of all an allusion to Sluse's method of tangents, "in which the equation is purged of irrational or fractional quantities"; then the remark, "I have no doubt that the gentlemen

From these extracts it follows that the contents of Newton's letter were unknown to him at that time (Oct., 1676).[29]

Regarding the verbal communications that Leibniz had from Collins during the second stay in London, Collins wrote to Newton from London on March 5, 1677 (1676 O. S.), that the representation of the roots of an equation by a series was discussed between them.

It is clear that Leibniz during his second stay in London had made himself more familiar with the results obtained by English mathematicians than he was before. The question now arises: What specially occupied his attention? What had particular influence upon his studies? It is seen that what Leibniz found in Collins's collection relating to algebraical analysis was new to him and excited his interest; also the verbal exchange of ideas between himself and Collins was upon the same subjects.

On the other hand, as regards the infinitesimal calculus, Leibniz obtained nothing during his second visit to London; he had made a progress, by the introduction of his algorithm into the higher analysis, beyond anything that came to his knowledge in London.[30] Also these algebraical results, at least for the next period, left behind no lasting impression; for among Leibniz's papers is to be found an extensive treatise, written on board the ship that carried him from London to Holland, wherein he considered the

I have just mentioned know the remedy that is necessary to apply"; then follows the rule for a quotient, and the remark that this will be sufficient for fractions; lastly the rule for powers, with the remark that this will be sufficient for irrationals. Later, he says, "This method has more advantage over all others that have been published than that of Slusius over all the rest, because it is one thing to give a simple abridgment of the calculation, and quite another thing to get rid of reductions and depressions."

Thus, after the sight of Newton's paper, his whole business has been to improve the method of Sluse.

[29] I read it quite otherwise; he has had information of some kind, whether from Oldenburg direct or from Tschirnhaus, while in Paris, and visits London with the express intent of seeing the original papers.

[30] See the third critical note, page 546.

fundamental principles of motion, in the form of a dialogue.[31]

It was in the letter to Oldenburg written from Amsterdam on November 18/28, 1676,[32] which Collins spoke of in the letter to Newton mentioned above, that Leibniz first refers to the subject of the problem of tangents, and remarked that the method of Slusius was not yet very perfect.[33]

KARL IMMANUEL GERHARDT.

CRITICAL NOTES ON GERHARDT'S ESSAY.

BY THE TRANSLATOR.

NOTE I. *The origin of Leibniz's "transmutation of figures."*

(Referred to in footnotes 12, 13, 15.)

In the manuscript, which follows under heading II, Leibniz appears to attach very considerable importance to the method of transmutation of figures, and to claim that he had originated it. This claim is not incontestible; indeed I am almost inclined to think it is a deliberate plagiarism to start with; but Leibniz has perceived

[31] Could this possibly have had its rise in an effort on the part of Leibniz to understand fluxions, or rather the idea of fluxions as he had found it in Newton's paper?

[32] In 1582, Gregory XIII had directed 10 days to be suppressed from the calendar, then in accordance with the Julian system of intercalation, in order to allow the error which had crept into the time of the vernal equinox, by which Easter-day was settled, to be put right. The Gregorian calendar was introduced into all Catholic countries the same year, in Scotland in 1600, in the protestant states of Germany in 1700, but not in England until 1752. At the same time the commencement of the legal year in England was altered from May 25 to January 1; thus we frequently find two years given for dates between January 1 and May 25; while there are two days of the month given for all months of the year. For instance, February 1673 in the new Gregorian calendar would be only February 1672 in the Julian, distinguished by the letters O. S. (Old Style); and this date was written February $167^{3}/_{2}$. Similarly the date November $^{18}/_{28}$, 1676, was the 28th of November in the New Style, and the 18th in the Old Style, the number of the year being the same, since the day did not lie between the 1st of January and the 25th of May.

[33] "*Methodus Tangentium a Slusio publicata nondum rei fastigium tenet.*" These are Leibniz's words; Gerhardt omits to translate the word *publicata*, which probably refers to the publication in the *Phil. Trans.* of 1672, by Slusius, of the rules of his method, illustrated by examples. Sluse had probably improved upon this before 1676, but there is no evidence on this point. It would seem as if the subsequent work by Leibniz, culminating in the manuscript of July 11, 1677, was largely an attempt to perfect the rule of Sluse *as a rule*, and that Leibniz, if ever, did not appreciate the idea fundamental in the calculus, namely that of rates, until very much later.

in it something which the original author did not. Can it by any chance be the case that, in conformity with several other instances of Leibniz's bad memory for details, he is confusing author and subject, when he speaks of "the great light that suddenly dawned on him, which the author had missed," the reference being to Pascal and the discovery of the differential triangle? Can it be that the true connection is that in considering the original work of the author of such transmutations of figures, he perceived the method for the arithmetical quadrature? For here he really has found a thing that the author missed though it was almost staring him in the face, his discovery being due to a habit that Leibniz had of writing down everything that he could get out of any particular figure or bit of work that he had in hand, whether it was relevant or irrelevant.

Wallis and Pascal had both hinted at the method, i. e., had used it in special cases, namely for proving the equivalence of the parabola and the spiral; and Leibniz was familiar with both these authors. Again, James Gregory had, in the words of Barrow (*Lect. Geom.*, Lect. XII, App. 3, foreword to Prob. IX), "set on foot a beautiful investigation about involute and evolute figures," i. e., polar and rectangular figures equal in area to one another. Of course, Leibniz may not have seen this work of Gregory until later; probably not, although in one of his manuscripts he gives a theorem of Gregory; this however does not count for much, for the very same theorem is given by Barrow (see my *Barrow*, p. 130) and we know that Leibniz had a Barrow in his possession. This book, judging by his words, "as in Barrow, *when his Lectures appeared*, in which I found the greater part of my theorems anticipated." Leibniz wishes to make his friends believe was the 1674 edition, and not the edition of 1670, which he bought on his first visit to London. Why did Leibniz wish to conceal this fact? I assert that the reason for doing so was the fear that seemed always to overshadow him, the fear of being accused of plagiarism, whether such was a true or a false charge. I am firmly convinced that Leibniz got his transmutation of figures from Barrow; to this conclusion I have only just come, it never having entered my head to look for it at the time that I wrote my articles for *The Monist* of October, 1916, April and July, 1917.

Before I bring forward my arguments, it is right to state as a preliminary that, just as in calculus nowadays we usually draw a curve with its convexity downward, and draw the tangent to meet the horizontal axis beneath the curve, so Barrow drew his curves with the concavity downward in many cases, mostly, I think, in order to fit the diagrams conveniently on the old-fashioned folding plates of diagrams, that in those days were added in batches at the end of a book (see a specimen I have given at the end of my *Barrow*); in other cases, he draws his figure on the left-hand side of the axis. Whichever figure he draws, he always did one thing, namely, he drew any supplementary figure he had need of *on the other side of his axis or base*. Leibniz almost invariably drew

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his curve on the right-hand side of a vertical axis, and supplementary figures on *the same side*. Hence, in the extract from Barrow given below, I am to be excused for failing to notice before what is more than a mere similarity.

In the following extract from Barrow (Lect. XI, Prop. 24), I have added Barrow's proof, which I thought unnecessary to give in my book; the figures given are Barrow's own on the left, which has been "up-ended" on the right; the latter is to be compared with the several figures by Leibniz.

Barrow's Lectiones Geometricae, Lect. XI, Prob. 24.

If DOK is any curve, D a given point on it, and DK any chord; also if DZI is a curve such that when any point M is taken in the curve DOK, DM is joined, DS is drawn perpendicular to DM, MS is the tangent to the curve, DP is taken along DK equal to DM, and PZ is drawn perpendicular to DK, so that PZ is equal to DS; in this case the space DZI is equal to twice the space DKOD.

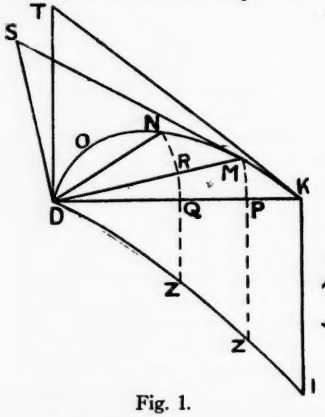


Fig. 1.

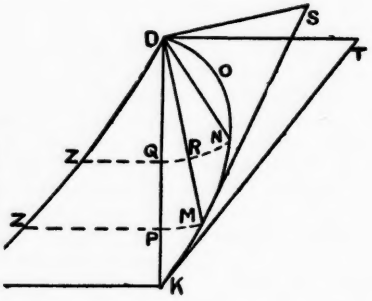


Fig. 2.

For let KP be considered to be indefinitely small, and let DT be perpendicular to DK and KT the tangent to the curve DOK. Then, drawing the arc MP, we have *as before,*

KP : PM = KD : DT = KD : KI, and hence KP . KI = PM . KD.

Take another small part PQ and, with center D, draw an arc QN through Q cutting the chord DM in R; then *as before,*

MR : RN = MD : DS, PQ : RN = MD : PZ, PQ . PZ = RN . MD;

and so on one after the other. Therefore, it is evident that the sum of all the rectangles KP . KI, PQ . PZ, etc., is equal to the aggregate of all the spaces PM . KD, RN . MD, etc.; that is, the space DKI = 2 times the space DKOD.

The words I have italicized refer to Prop. 22, in which he uses

a similar though rather more complicated figure to reduce a polar area to a rectangle *of which one side is a given straight line*, and explains that the reasoning depends on the fact that the line DK is divided into infinitely small parts. Compare the words I have italicized with the description of Leibniz's method: "the areas of these triangles *being proportional to lines.*

Further, Barrow proceeds in Prop. 25 to prove the equivalence of the spaces formed (i) by applying each MS to the base and (ii) by applying each chord to the arc, previously rectified. And he winds up with the words: "Should any one explore and investigate this mine, he will find very many things of this kind. Let him do so who must, or if it pleases him."

This all suggests that Leibniz *did* explore this mine, that he did *not* invent the method of transmutation of figures for himself,

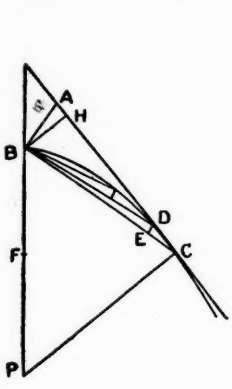


Fig. 3.

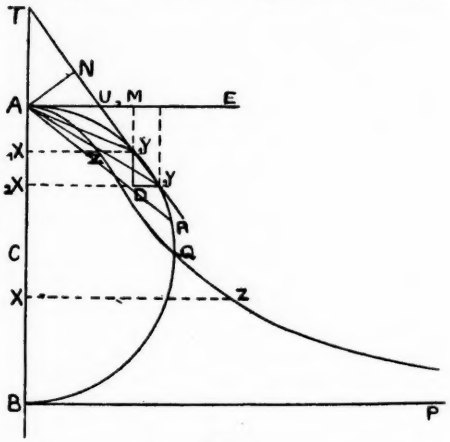


Fig. 4.

that he *did* find very many things of this kind, and that Barrow had *missed the arithmetical quadrature* construction; this Leibniz obtained through his regular practice of working every mine right out, to keep up Barrow's simile. Further comment is needless, I think, after a comparison of Barrow's figure (the up-ended version) with the figures of Leibniz given above.

Fig. 3 occurs in a manuscript November 21, 1675, which according to Leibniz is at least a year after he had discovered the arithmetical quadrature; and yet it has a heading, "A new kind of Trigonometry of indivisibles, etc." In this figure it is to be noticed that he has the perpendicular to the chord BC, agreeing with Barrow's DS and DT, but has not the tangent at the vertex that was necessary for the demonstration of the arithmetical quadrature. In the working in connection, he considers the similarity of all the tri-

angles possible, and notes as one point that "*the sum of all the triangles or the area of the figure is equal to the products of the AB's into the CE's,* which is Barrow's proof of Prop. 24 above.

Fig. 4 is the figure given in the *Historia* (see *Monist* for Oct., 1916), in connection with the explanation of how he found the area of the circle. Notice the difference between this figure and the one given in the manuscript that follows under the heading II, also that the description there given of the way in which he was led to it is much more natural. This is probably the true version, for the use of the notation B, (B), ((B)), points out that it was written at a comparatively early period, before Leibniz had adopted the prefix notation, $_1$B, $_2$B, $_3$B. In the account in the *Historia,* to which Fig. 4 applies, Leibniz says, "he once happened to have occasion to break up an area into triangles formed by a number of straight lines meeting at a point, and he perceived that something new could be readily obtained from it." I suggest that the occasion was most probably while he was digging in Barrow's mine! This is the reason why he has in the *Historia* given the figure more according to his usual practice, and different from the figure in the earlier manuscript, which is too much like a copy of Barrow's (query, where did Barrow get it from?). With regard to the figure and proof in the manuscript which follows, we find that the reasoning there given is unsound, unless Gerhardt has given us a slightly erroneous diagram; for Leibniz apparently does not perceive that the ordinates BA, which are equal to the corresponding CE, *must pass through the respective points D,* before he can say that one figure is double the other. Hence I conclude that at the date of this manuscript, the demonstration was imperfect and that he had no proof until he dug in Barrow's mine; in support of which conclusion I will quote from the *Recensio,* mentioned in footnote 22. "This quadrature, composed in the common manner, he began to communicate at Paris in the year 1675. The next year he was polishing the demonstration of it, to send it to Mr. Oldenburg, in recompense for Mr. Newton's Method, as he wrote to him May 12, 1676; and accordingly in his letter of August 27, 1676, he sent it, composed and polished in the common manner." This polishing, I take it, consisted in making the slight but important alterations in the demonstration and figure, from those given in the manuscript II that follows, to those given in the *Historia.*

What had he then got in July 1674, when he wrote to Oldenburg saying that he had got a wonderful Theorem, which gave the area of a circle, or any *sector of it exactly,* in a series of rational numbers? Or, when in the October following, October 26, 1674, he wrote to say that he had found the circumference of a circle in a series of very simple numbers; and also by the same "method" (a favorite expression of Leibniz) *any arc whose sine was given*? It was impossible that Leibniz could have had the two things that I have italicized; or at least, the latter was impossible to him, be-

cause the only way for him to obtain it *exactly*, i. e., to know the law of his series, was as yet unknown to him; unless we are to assume, contrary to his assertion, that the binomial theorem was known to him, which would involve his also having seen or been told about other parts of Newton's work. The only way open to Leibniz was to find the square root of $1-x^2$, and then its reciprocal by division; and this would not give him the law of the series, even if we assume that his knowledge of integration was sufficient to enable him to proceed any further. From his manuscripts it does not seem that even up to Nov. 1675 he had any further knowledge of integrations than that omn.$x=x^2/2$, and omn.$x^2=x^3/3$; but as he says that he knows the latter from the quadrature of the parabola, there is some possibility that he might have been able to integrate every integral power of the variable from his reading of Wallis and Mercator.

However, there is the strongest probability that he had not got any proof for the two things italicized, and that the quadrature was in the same category. Where then had he obtained it? We find that in December, 1670, Gregory had found out for himself Newton's method of series; and two months later, February 15, 1671, sent several theorems to Collins, one of which was that now known as "Gregory's series." "And Mr. Collins was very free in communicating what he had received both from Mr. Newton and Mr. Gregory, as appears by his letters printed in the Commercium" (from the *Recensio*). One can imagine that Oldenburg would be one of the first to receive the information, and that for a certainty it would be passed on to Leibniz. I think then that Leibniz perceived that by putting $x=1$ in Gregory's series, and making the radius of the circle equal to unity, he could get an arithmetical quadrature; from that time onward he looked for a proof by pure geometry, and found it after reading Barrow's proposition referred to above; if we assume the possibility of integration of integral powers, it was an easy step to find that the series he had to integrate was $y^2/(1+y^2)$, and all he had to look for on his figure was a line of this length. This very well accords with the description of the way in which he found his demonstration, as given in the manuscript which follows under the heading II.

Lastly, in connection with the suggestion that I have made above, namely, that Leibniz had another method for his arithmetical quadrature than those he has given, there is one method that is bound up with the change that he made from the Pascalian characteristic triangle which he used at first, to the Barrovian differential triangle (see my note in *The Monist*, Oct., 1916, p. 615). In Example 5 of the method of the differential triangle (see my *Barrow*, p. 123), Barrow has found the subtangent for the curve $y=\tan x$, from a consideration of the figures on next page, and finds that

$$t=\frac{rr}{rr+mm}m.=\frac{\overline{CB}^2}{\overline{CG}^2}.BG=\frac{\overline{CK}^2}{\overline{CE}^2}.BG\ .$$

where r is the radius of the circle, m is the ordinate MP, which is equal to BG, and t is the subtangent TP.

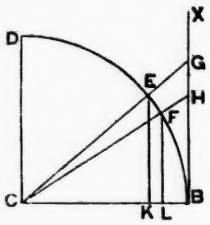


Fig. 5.

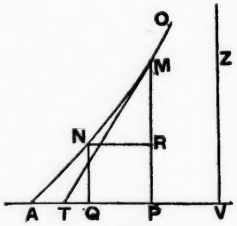


Fig. 6.

Now if we put the radius equal to unity, and for the ratio t/m substitute what was known by Leibniz to be equal to it, namely, QP/RM or EF/GH (by construction), we have the sum of all the EF's is equal to the sum of ordinates equal to CK^2 (radius = 1) applied to G at right angles to BG. Analytically, calling BG z, we have

$$\text{arc BE} = \text{sum.omn.} \frac{1}{1+z^2} \text{ applied to the line } z;$$

hence by division

$$\text{arc BE} = \text{sum.omn.}(1 - z^2 + z^4 - z^6 + \text{etc.})$$
$$= z - z^3/3 + \text{etc.}$$

I can hardly see how Leibniz could have missed this with his analytical mind, even although Barrow has missed it; but there is a strong probability that at the time of writing, Barrow had not seen the quadrature of the hyperbola by Mercator, and, if he had, such algebraical work would not have appealed to him at all.

As far as I can make out, there is only one other alternative, which involves a direct contradiction of Leibniz's own statement; that is that his proof was not by the transmutation of figures in the first instance. Color is lent to this view by a letter of Leibniz and other papers, quoted by Sloman (pp. 131ff, in the English edition of the work referred to in footnote 23); also even by a passage in the *Historia* (see *Monist,* Oct., 1916, p. 599), where, while giving the story of the discovery of the arithmetical tetragonism, Leibniz distinctly hints at an algebraical method; for he says immediately afterwards, "The author obtained *the same result by the method of transmutations,* of which he sent an account to England." This reads as if he had another method in addition to the method by transmutations.

Let us consider this algebraical method. To square the circle, Leibniz has to integrate $\sqrt{(1-x^2)} = y$, say; let $y = 1 - xz$, then $y = (1-z^2)/(1+z^2)$, which is rational; moreover, he would also have been able to have substantiated his statement that at this time he

also had a proof of the series for the arc whose sine was given, for which he would only have had to integrate $1/\sqrt{(1-x^2)}$. But one cannot conceive that Leibniz had any means of expressing the element of z in terms of the element of x. Geometrically, he was incapable of it, without using Barrow's infinitesimal method; and of this we find the first instance in a manuscript dated November 1, 1675. Algebraically, he could not, for at this same date he could not differentiate a product. How then are we to account for the fact that he says he has a method for demonstrating both series for the arc, given the sine or the tangent? I think I can answer this. Many times we find assertions made, not only by Leibniz in those times, but by others in other times, of the possession of discoveries, when all that the assertor has is the idea of how they may be obtained. Thus, in the passage quoted, the concluding statement is, "and thus again all that remains to be done is the summation of rationals." So that if we accept this alternative we are bound to come to the conclusion that Leibniz did not yet recognize, what he ought to have done from the work of Pascal, that an area was not a mere summation of lines, but of rectangles formed by these lines ordinated at certain definite points along a straight line. That is to say, he did not recognize the fundamental principle of integration, namely, the importance of the factor dx or dz. When he had to write out his proof he found that the summation of $(1-z^2)/(1+z^2)$ or its reciprocal was beyond him; or rather that the series he found by Mercator's method was not correct; he had to resort to the geometrical proof, of which he got the idea by digging in Barrow's mine, as above; he found that this would not work for the other series; and consequently he dropped all claim to the second series. In his letters of 1676, therefore, we find him offering to send Newton the proof of his quadrature in return for the method of proof of the series for the arc when the sine is given.

Thus I come to the conclusion that Leibniz obtained these series in some way by correspondence, thought he had got a proof of his own, (which turned out to be incorrect), and much later did obtain a proof of his arithmetical quadrature by the transmutation of figures, *after obtaining the idea from Barrow.* As the special case, when x = the radius, had not been specifically mentioned by Gregory, Leibniz considered that he had a right to claim it, more particularly as he thought he had devised a proof for it, if it was necessary to produce one; for of course, Gregory had given no proof according to the usual custom of the time. Then, when he did find a proof, after having found that his original idea was hopeless, one can hardly blame him for sticking to his claim.

NOTE 2. *On the introduction of the Leibnizian algorithm.*

(Referred to in footnote 21.)

The two passages in which the signs for integration and dif-

ferentiation are respectively introduced occur in the manuscript of October 26, 1675.

i. "It will be useful to write $/$ for omn., so that $/l = \text{omn. } l$, or the sum of the l's."

ii. Not for some time is the sign for differentiation introduced, and then in these words: "I propose to return to former considerations. Given l and its relation to x, to find $/l$. Now this comes from the contrary calculus, that is to say if $/l = ya$. Let us assume that $l = ya/d$, or as $/$ increases, so d will diminish the dimensions. But $/$ means a sum, and d a difference. From the given y, we can always find ya/d or l, or the difference of the y's. Hence one equation may be changed into the other,....."

Now of these the introduction of the symbol for integration can no more be called an invention than the use of Σ to stand for "the sum of all such terms as." It was simply, as Leibniz himself says, a convenient and useful abbreviation for sum.omn. or omn. It is nothing more or less than the long s then in general use; indeed it was so thought of by contemporary mathematicians, Newton for one at any rate, for we find in the *Recensio* the passage, "Mr. Leibniz has used the symbols sx, sy, sz for the sums of ordinates ever since the year 1686." This may have been an instance of prejudice, or perhaps the printers of the *Phil. Trans.* may not have had an integral sign in their fonts of type; but it shows up the fact that the English accepted it as the initial letter of the word "summa."

Now let us consider the introduction of the letter d. Gerhardt says that it resulted as antithesis to the sign $/$. How he can possibly derive this from the context I cannot surmise. I am well aware that in another passage he was unable to assign a meaning to the introduction of a letter, which was, to me, clearly used for the simple purpose of keeping the dimensions correct. We have this use again in the present passage. Leibniz knows that the sum of the lengths, $/l$, is an area: hence taking y to represent a length, given in terms of x, he introduces the *length* denoted by a to give with y the area of a rectangle. Therefore he argues that l must be an area divided by a length, and he writes $l = ya/d$, where d is *another length, introduced to keep the dimensions correct.* This is clear from the sentence that follows next: "so will d diminish the dimensions."

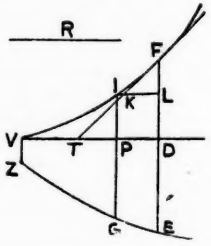
So far the sequence of ideas is easy to follow, and there is not the slightest trace of any concept of differentiation, nor, if the l's are ordinated to any axis, any trace of a connection between d and an element of that axis. The difficulty begins with the next sentence: "But $/$ means a sum, and d a difference." The first idea that strikes one is that this was added later, after that he had found out the connection between the inverse-tangent problem and quadratures. Gerhardt gives no suggestion on the point, so until the paper can be reexamined for small details like differences in the ink or character of the writing this idea will be disregarded. The next is that about this time he was reading Barrow, and then one is at once reminded

of Lect. X, Prop. 11; this is the proposition in which Barrow proves that differentiation is the inverse of integration. If we consider this in the manner of Leibniz, we get the equivalent that is set down on the right-hand side below:

BARROW

Let ZGE be any curve of which the axis is VD; and let ordinates applied to this axis, VZ, PG, DE, continually increase from the initial ordinate VZ; also let VIF be a line such that if any straight line EDF is drawn perpendicular to VD, cutting the curves in the points E. F, and VD in D, the rectangle contained by DF and a given length R is equal to the intercepted space VDEZ; also let DE: DF = R: DT, and join DT. Then TF will touch the curve VIF.

Cor. It should be observed that DE. DT = R. DF = area VDEZ.



LEIBNIZ

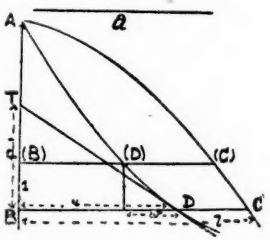
Let AC be a curve, whose axis is AB, and let the ordinate AB be l;

let AD be another curve, having the same axis, and let its ordinate DB be called y;

let this curve AD be such that the area ABC, i. e., all the l's or $/l.$ is equal to the product of BD and a fixed line, i. e., equal to ay;

then, taking B(B) equal to unity, we have $l = aw$, where w: B(B) = DB: BT, or $w = y/d$, i. e.,

$$l = ay/d.$$



We thus see that the d that results as the "antithesis to the integral sign" (*als Gegensatz....sich ergab*), is not a difference at all, but the subtangent; it is y/d or w (on account of B(B) being taken as unity) that is the difference between the ordinates y. But there is not the slightest trace of the idea of differentiation: this is made more manifest by the work which follows, which is based on his idea of obtaining independent equations, and eliminating all variables but one and thus reducing the problem to a quadrature. And yet he seems to perceive from the equation that gives the difference of the y's as a *quotient*, that in some unintelligible way a division means a difference. Later therefore we find him trying to find an interpretation of d as an operator, whether he writes it

in front of his y, or as a denominator; namely, when he considers what value he is to assign to $d(xy)$. I venture to assert, unless we assume that Leibniz is considering this proposition of Barrow's, that there is no possible connection to be made out between the several sentences of this passage. Also that in no sense can this introduction of the letter d be looked on as an algorithm with any idea in it of differentiation.

I am well aware that in the above I have adduced no positive proof that my idea is correct; I have not had the advantage of Gerhardt in seeing these manuscripts. But I have honestly tried to find other ways of explaining the circumstances that lead from y/d as a *quotient* to dy as a *difference*, and I can find none other that is feasible than that given above, namely, that, perhaps by accident, Leibniz uses d for the subtangent (instead of the usual t), and perceives from such a figure as the above (which of course I do not intend to say he has given) that y/d (where d is the subtangent) works out the same as dy (when dx is taken to be unity); in other words the subtangent d is equal to $y/(dy/dx)$.

NOTE 3. *On the progress made by Leibniz before November, 1676.*

(Referred to in footnote 30.)

The remark made by Gerhardt that Leibniz "had made a progress, *by the introduction of his algorithm into the higher analysis,* beyond anything that came to his knowledge in London," is, to say the least of it, a matter of opinion. From a study of the six manuscripts, that Gerhardt has given us, that bear dates between that of the introduction of the integral and differential symbols (Oct. 26, 1675) and that of his return to Germany, via Amsterdam (after Nov., 1676), I fail to see that there is very much occasion for the main part of the above statement, namely, that the progress made by Leibniz was at all greater than anything that came to his knowledge in London; as for this progress, if for a moment we assume its superiority, being due to the reason set in italics, I fail to see that Gerhardt has any grounds whatever for such a statement.

The six manuscripts in question have been given, translated into English and annotated in *The Monist,* April, 1917; but for convenience I here add a precis of them.

i. Nov. 1, 1675. A continuation of the work on moments about axes; the new symbols do not occur, *omn.* being still used. He has now read Wallis, Gregory and Barrow, in addition to Cavalieri and St. Vincent; he speaks of his theorem of breaking up a figure into triangles as bringing out something new; the whole tone of this manuscript is in the main Pascalian.

ii. Nov. 11, 1675. He successfully obtains a solution of the problem of finding a curve such that the rectangle contained by

the subnormal and ordinate is constant. This he considers to be "one of the most difficult things in the whole of geometry." He uses the integral sign, and the denominator d; but neither integration nor differentiation, the fact that $y^2/2d=y$, being taken from the "*quadrature of the triangle.*" In verifying his result he quotes Slusius's Rule of Tangents. Further on, he has the note that x/d and dx are the same thing, though there is nothing to show why he comes to this conclusion; see the last critical note. He also comes to the conclusion that $d(xy)$ is not the same as $dx.dy$; but in the last bit of work in this manuscript he uses special letters for the infinitesimals, showing that he has been trying to find the effect of d as an *operator,* or perhaps trying to find the reason of the equality x/d and dx. He has failed to solve a problem, which results in the differential equation, as we should now write it, $x+y.dy/dx=a^2/y$, or as Leibniz has it $x+w=a^2/y$; although he gives an incorrect solution, which he asserts to be true. This time he does not attempt to verify his solution, the reason being obviously that he is unable to do so, because one side of his equation is a product. As a matter of fact, I have it on the authority of Professor Forsyth that there is no solution of this equation in elementary functions; or at least he says that he has been unable to find one, which I take it comes to the same thing. The one advance that can be found here is the appreciation that squares and products of infinitesimals can be neglected, as he has doubtless found in reading Barrow. It is worth noting that he now uses the differential triangle in Barrow's form instead of the form he says he got from Pascal.

iii. Nov. 21, 1675. In this manuscript he sets himself another problem, which he fails to solve; the curve required is logarithmic, and this fact even he fails to bring out. In generalizations that arise from the consideration of his problem he obtains $dx\,y=xy-x\,dy$, in a more or less analytical manner; but immediately afterward states that nothing new can be obtained from it; he has already obtained this formula by his consideration of moments, geometrically; and he does not appreciate the advance there is in obtaining it algebraically. The manuscript concludes with a consideration of the figure by means of which it is generally supposed that he affected his arithmetical quadrature. This is very remarkable on account of the heading, which reads, "A *new* kind of Trigonometry of indivisibles, by the help of ordinates that are not parallel but converge." What I refer to is the use of the word new, which I have here italicized. It is to be observed that the diagram and the results are almost identical with those of *Barrow,* Lect. XI, Prop. 22-24 (see the first critical note). He concludes by a reference to the trochoids,

which shows that he is still under the influence of Pascal, if indeed he is not still studying his works.

iv. Nov. 22, 1675. He returns to the subjects of the previous day. But there is here no mention of the signs of integration or differentiation.

v. June 28, 1676. Here we have a certain advance, for there occurs the statement: "The true general method of tangents is by means of differences." While he uses dy and dz for the elements of y and z, he uses β for the element of x; the rest of the work is merely Barrovian in principle. This mere substitution of dy and dz for the special letters used by Barrow for the same things can hardly be called progress. What progress there might be is barred by the use of equations with three or more variables in them.

vi. July, 1676. The remark on the last manuscript is corroborated by the contents of this manuscript. Leibniz asserts that he has solved two problems, of which Descartes had alone solved one, and owned that he could not solve the other. The truth is that he has not solved the former, which was fairly easy, only given an alternative construction which is, if anything, more difficult to carry out than a construction from the original data for the curve. The latter he gets out in a hazy fashion ("....which belongs to a logarithmic curve"). This conclusion he comes to after several erroneous steps of reasoning; whereas the solution stared him in the face about a quarter of the way through the work, where he has the equation $c\,dy = y\,dx$, if he could have integrated dy/y with certainty. The failure I think arises from the study of Pascal, who lays it down that only one of the variables can increase arithmetically, and Mercator's work has been with y increasing arithmetically, and Leibniz has already considered that the x is increasing arithmetically (See my notes on this manuscript in *The Monist* for July, 1917).

Throughout the whole of these manuscripts, he makes no progress, because he is hampered by the idea of keeping one of his variables increasing uniformly; he seldom uses his algorithm for differentiation; and when he does do so, it is merely a substitution of dx, etc. for the special letters used by Barrow. In fact these manuscripts appear to me to be the records of his work on the textbooks of his study, Pascal, Wallis, Gregory, and Barrow; and we see him trying to fit the matter and methods found in them into his own ideas and notations. It is not until November, 1676, when he has arrived on the Continent, after having seen Newton's paper, that we have any Differential Calculus; even then some of the standard forms that he gives are not quite correct; on the other hand, he gives the method of substitution to differentiate an irrational, though he uses the Barrovian method to differentiate the general equation of the second degree, merely using dy and dx instead of

Barrow's special letters. It is not until July, 1677, that he is able to give anything like an intelligible account of the differentiation of products, powers, quotients and roots. Lastly I doubt if Leibniz ever did really appreciate the Newtonian idea that dy/dx was a *rate*, or else the example he gives of the use of the second and third differentials in his answer to Nieuwentiit would not have contained so many ridiculous errors.

TRANSLATIONS OF THE MANUSCRIPTS

Alluded to by Dr. Gerhardt.

I.

Scientific memoranda of the visit to England at the beginning of the year 1673.

When at the beginning of the year 1673, I accompanied his Excellency the Ambassador of Mainz, Baron Schörnborn, a nephew (on his father's side) of the Elector, from Paris to London, although I stayed in England scarcely a month, among various distractions, I still gave attention to increasing my knowledge of philosophy; for at that time the English held a high reputation in this subject.

To set out a long minute record of daily happenings is useless on account of its inequality; for the fortune of all the days was not the same; indeed the points worth remarking heaped themselves up one day, and the next gaped with emptiness. For this reason perhaps it will be more satisfactory to go by heading of subjects, one remark recalling another as it were.

The principal heads for the subjects noted may be taken as Arithmetic, Geometry, Music, Optics, Astronomy, Mechanics, Botany, Anatomy, Chemistry, Medicine, and Miscellaneous.

ARITHMETIC. The line of proportions or Gunter's lines or the double scale. Logarithmotechnia or compendium for calculating logarithms. To recognize square numbers from non-squares by their end figures. Morland's machine.

ALGEBRA. Substance of English algebraical work of 27 years. Algebra of Pell. At first few rules, but lots of selected examples. Renaldinus not thought much of in England.

GEOMETRY. Tangents to all curves. Development of geometrical figures by the motion of a point in a moving line.

MUSIC. Its universal character. System of Birthincha. Vossius will publish Music.

OPTICS. They told me of a certain phenomenon that Barrow confessed that he was unable to solve. The difficulty of Newton hitherto unsolved, Father Pardies giving it up. Hook adheres to a catadioptric instrument of 9 feet, because for another of 50 feet movement inconveniences them. The secret of the largest aperture which can be given to microscopes is primarily as great as the distance of the object.

ASTRONOMY. Arrangement of Hook for observing whether the earth at any time sensibly approaches or recedes from the fixed stars, from which it can be judged that it is not in the center of the universe; he erected it in a fine tube set perpendicularly, and observed the stars that are vertically overhead. He, lying flat on his back, observed their dimensions most exactly.

CHEMISTRY.

MECHANICS.

PNEUMATICS.

METEOROLOGY.

HYDROSTATICS.

NAVIGATION.

MAGNETISM.

PHYSICS.

BOTANY.

ANATOMY.

MEDICINE.

MISCELLANEOUS.

II.

[This manuscript is very lengthy, the translation running to

about 6000 words, of which the first 5000 are written as a concise history of all the great geometers and their works, that are antecedent to Leibniz himself. This part is quite unimportant for the purpose of estimating the part that was played by Leibniz, and it passes my comprehension why Gerhardt should give it at length, while he has condensed the other two, which are really important. Hence, in what follows, I have given a precis of the first 5000 words, with here and there quotations, in which Leibniz has something to say that is either critical of the work of others, or a claim to superior knowledge or better method of his own. The last part, which purports to be the history of his arithmetical quadrature, together with his claim to the surpassing value of his achievement, I have given in full.]

(Precis). Geometry is a modern thing, probably due to the Greeks. The great name among the Ancients is that of Archimedes, who first used indivisibles; this use was more profound than that of Cavalieri, but the method became lost. The name of Apollonius must not be altogether omitted.

The learning of the Greeks passed on to the Arabs, who conquered them; among these we have Alhazen, and a certain Mahomet, who gave the formula for the general quadratic.

This brings us to the cubic and biquadratic equations, which were solved in the sixteenth century. The cubic is due to one Scipio Ferreus of Bologna; one of his pupils set the solution as a challenge after Scipio's death; Tartalea took up the challenge, found a solution and told his friend Cardan; the latter extended it and published it without the consent of Tartalea. Vieta, Descartes, and Ferrarius gave the solution of the biquadratic. But even Descartes and Vieta failed at equations of higher degrees. With regard to the work of Descartes, Leibniz remarks that "its origin [that is, of the method of solution] was a widely different and more fertile spring; and if Descartes had only recognized this, he would have rendered the discovery of Scipio more general and carried it to further heights. *But what has befallen me in this connection I will say in another place.*" Leibniz further remarks that the method of Descartes fails to give the roots of equations of higher degree, although the quality of the roots may be learned through it. "I will show in another place that the *reason for this is clearly known to me from the most fundamental principles of the art, and that I have established an extremely easy method, and one that is adapted too for enlarging science, by the many things that follow from it.*"

In the seventeenth century, Leibniz goes on to say, after Archi-

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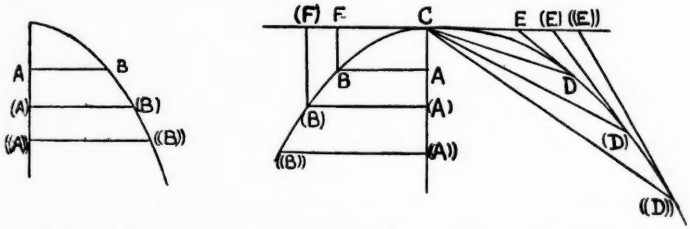
medes and Galileo's several times and influence are gone by, there is no writer from whom more is to be learned than from Descartes; and yet he is "*unable to pass over certain boastful remarks that he makes, by which the less experienced among us may be led into error.*" Descartes had said that by his method every geometrical problem could be reduced to the finding of the roots of equations. Leibniz remarks that this shows Descartes's ignorance of the matter. "For when the magnitude of curved lines or the space enclosed by such is required (which happen more frequently than perhaps Descartes thought, since he had not applied himself sufficiently to the 'mechanics' of Galileo), neither equations nor Cartesian curves can help us, and *there is need of equations of a totally new kind, of constructions and new curves, and finally of a new calculus, given so far by nobody, of which, if nothing else, I can now give certain examples at least, which are remarkable enough.*".... *I have mentioned these things so that men may understand that there are certain methods in Geometry, for which they may look in vain in the works of Descartes.*"

Returning to geometry purely, Leibniz next mentions the work of Galileo, Cavalieri (whose method he considers is rough and limited in extent), Torricelli, Roberval, Pascal, Wallis, Huygens, and Slusius, as contributors to the new geometry. He considers that a new epoch opens with the work of Neil and van Huraet (on rectification of curves), James Gregory, and Brouncker. "Finally Mercator gave a general formula for the area under a hyperbola." He claims Mercator as "an eminent German geometer"; but rather decries his discovery as being an easy one, on account of the ordinates working out as rational in terms of the abscissa. "But it was not so easy to give the magnitude of the circle, and its parts, expressed as an infinite series of rational numbers;.... for the circle, however you treat it, has ordinates that are irrational. However I, as soon as I had found a certain very general theorem, by means of which any figure whatever could be converted into another that is quite different from it, but yet of equivalent area, set to work to try whether the circle could not be converted in some way into a rational figure; and the thing came out beautifully;.... it will be worth while here to give a short account of the matter."

(In full). Nearly everybody who has up to now treated of the geometry of indivisibles has been accustomed to break up their figures into rectangles or parallelograms only by means of ordinates

parallel to one another. But the reasoning of Desargues and Pascal always pleased me very much; these in Conics, as we can call them in general, include under the name of ordinates not only parallels, but also straight lines meeting in or converging to a point, especially when parallels are included under the name of converging, by saying that the point of convergence goes off to an infinite distance. Thus while others only consider parallel ordinates, and have broken up their figures into parallelograms AB(B)(A), (A)(B)((B))((A)), in the way that Cavalieri does, I employ converging lines and resolve the given figure into triangles CD(D), C(D)((D)), and at once draw another figure of which the ordinates AB, (A)(B), etc., are proportional to these triangles.



Now this is the case if the AB's are equal to the CE's where it is supposed that the straight lines DE are tangents to the given curve; for in that case, as I will show below, it will come out that the space B(B)(A)A will be double of the segment C(D)DC, and for any figure such as C(D)DC another that is equivalent to it can be drawn. Now, supposing that the curve D(D)((D)) is circular and that CA is a part of the diameter, then, calling CA or FB x, and CF or AB y, and the radius of the circle unity, calculation will show that the value of x is $2y^2/(1+y^2)$. Thus the ordinate FB or x can be expressed rationally in terms of the given abscissa CF or y. Such figures as these, in which the ordinates can be expressed rationally in terms of the abscissae, I call rational. Thus we have drawn a rational figure equivalent to the circle, and this will be soon seen to be sufficient to give the arithmetical quadrature of the latter. For, from the sum of a geometric series of an infinite number of decreasing terms that is well known to all geometers, it follows that $y^2-y^4+y^6-y^8+y^{10}-y^{12}+$ etc. to infinity is the same as $y^2/(1+y^2)$, i. e., the same as $\frac{1}{2}x$, if only we understand that y is a quantity that is less than the radius, or unity. Now, since we have to collect together the infinite number of $\frac{1}{2}x$'s into one sum, in order

to obtain the quadrature of half the figure C(F)(B)BC and what it comes to, namely, that of the circle; so also have we to collect together the infinite number of series $y^2-y^4+y^6-y^8+y^{10}-y^{12}+$etc., into one sum, and this by the method of indivisibles and infinites can be done without difficulty. For, suppose that the last y, which in general is taken as C(F), to be b, then the sum of every y^2 will be $b^3/3$, and of every y^4 will be $b^5/5$, and of every y^6 will be $b^7/7$, and so on; hence, the sum of the infinite number of $\frac{1}{2}x$'s, or of the series $y^2-y^4+y^6-y^8+y^{10}-y^{12}+$ etc., i. e., the area of half the space C(F)(B)BC, will be $b^3/3-b^5/5+b^7/7-b^9/9$ etc. From which, by the help of ordinary geometry, it can be easily deduced that the square on the diameter is to the area of the circle as 1 is to $1/1-1/3+1/5-1/7+$etc.; also speaking in general, supposing b to be the tangent, then the arc is $b/1-b^3/3+b^5/5-b^7/7+b^9/9-b^{11}/11+$ etc. Hence it now follows that any one without the help of tables and continual bisections of angles and extractions of roots can approximate to the magnitude of the arc to any degree of accuracy desired, so long as the tangent b is a little less than the radius; so that if we take the tangent to be a little less than the tenth part of the radius, the arc may be obtained with sufficient accuracy. Let us take the tangent to be a tenth part of the radius, then if we want the arc, it will be

$$\frac{1}{10}-\frac{1}{3000}+\frac{1}{500000}-\frac{1}{70000000}+\frac{1}{9000000000}=\text{etc.};$$

and reducing all to a common denominator, and adding the numbers into one sum (for it is not worth while going any further), then the arc will be a little greater than 518027821302775/5197500000000000, and the defect of this value from the true value will be less than the 1/1000000000000 part of the radius. For if we do not subtract the last term, 1/1100000000000, the value would be too great, and if we do subtract it, the value is less than the true value, therefore the error is less than 1/1100000000000, and thus is less than 1/1000000000000.

It is seen how exactly it comes out with such easy calculation involving only additions, subtractions and multiplications, to an extent that is not obtainable with tables. Also if the ratio of the tangent to the radius is anything else, the arc can similarly be found, and this is especially easy when it can be expressed in decimal parts. Again, since now the ratio of the circumference to the radius is given in numbers of any required degree of accuracy, by this also

the ratio of a given arc to the circumference is given, and thus also the quantity of angle for a given tangent will appear with any required degree of accuracy. In this way tables may be corrected, supplemented, or, if need be, enlarged, with no great trouble. Any one who will just remember this fairly easy rule will be able without tables to attain to any required degree of accuracy with very little labor. How great an acquisition this is to geometry, I leave it to those who understand to estimate.

Critical Note.

It is difficult to see the object that Leibniz had in writing this long historical prelude to an imperfect proof of his arithmetical quadrature, unless it can be ascribed to a motive of self-praise. This suggestion would seem to be corroborated by the claims that Leibniz makes in the parts where I have quoted his own words in italics in the precis, and by the concluding sentence of the translation given in full. Even if this is so, there may be some plea of justification put forward; for Leibniz appears to have been a man impelled by many contradictory motives, but these I think can all be traced back to one origin. The time in which he lived was a time of great discoveries in geometry; Leibniz knew in his soul that he had it in him to be one of the great men in this branch of learning, but as truly recognized his great disability due to his lateness in starting, and felt that his only chance was to belong to the very exclusive set who corresponded with one another; he saw that the only way of entering this set was to do something brilliant. This may be taken as some excuse for any self-praise that we find, and to a less extent for his, to my mind, undoubted plagiarisms. With regard to the behavior of Leibniz, when charged with these plagiarisms, Sloman is not beyond calling Leibniz a liar point-blank; I prefer to call his statements perversions of the truth, made under stress of circumstances, so that *his reputation as a great and original thinker should not suffer.* For instance, to explain what I mean, I will take the statement of Leibniz to de l'Hospital that he owed nothing to Barrow. As I have said in another place, from one point of view, the point of view that Leibniz would take for the *purposes of this letter,* Barrow would be a hindrance rather than a help to Leibniz, in the formulation of his *algebraical* calculus, *after he had once absorbed all the fundamental ideas.* That is, it would seem that Leibniz always tries to tell the truth, but to put it in a form that to the uninformed reader will convey quite a wrong impression. Another example of this juggling with words and phrases is given by Sloman, in the shape of a letter from Leibniz, dated August 27, 1676, *and the first draft of the same*; these two read together are very much the same, but read apart convey a totally different impression.

A second characteristic of Leibniz may also be traced back to

his desire to make up for his lateness in starting; that is, the sometimes ridiculous claims that Leibniz makes to discoveries, or rather hints at having made them. An instance is given in the *Historia* (see *Monist*, Oct., 1916, p. 599). "It is required to form the sum of all the ordinates $\sqrt{(1-xx)}=y$; suppose $y=\pm 1\mp xz$, from which $x=2z/(1+zz)$, and $y=(\pm zz\mp 1)/(zz+1)$; *and thus again all that remains to be done is the summation* of rationals." Unless we assume that Leibniz never understood in all his life what we now call the change of the variable in integration, which to me seems rather far-fetched, the only reason why this should have been allowed to appear in a tract that was certainly written after 1712, is that Leibniz had never attempted this summation; he had set this down in 1674 and 1675 as a method of quadrature for the circle, not at that time having perceived the importance of the factor *dz*, or, in other words, the way in which the ordinates should be ordinated; for as I have already pointed out, at that time Leibniz could not have found *dz*, since he could not differentiate a product. This goes to prove that his reading of Pascal was not of the profoundest; for Pascal is very careful over this point, going to the trouble of calling the *y*'s ordinates when drawn through the points of equal division of the base, and sines when they are drawn through the points of equal division of the arc. Probably to this characteristic is due the claim, set in italics in the manuscript above, with respect to equations of higher degrees. *He thought* he had a general method, which he had not time to verify by particular examples, and so find that his claim was erroneous. For surely this cannot be read as a claim to the Tschirnhausian transformation and the expression of a quintic in the canonical form $x^5+px+q=0$.

The date of the above manuscript is almost certainly antecedent to the manuscript that Leibniz got ready for the press, *De Quadratura*; hence his claim to be able to give examples of the calculus, except for integral powers which had already been done by Wallis, is without foundation.

With regard to the arithmetical quadrature itself, the great importance of it in the estimation of Leibniz is apparently in the correction and enlargement of tables; this claim, as Leibniz puts it, is ridiculous, although it could be so used by first constructing tables for angles whose tangents are given. But Leibniz, after giving a calculation true to twelve places of decimals, states that "the ratio of the circumference to the radius is now known," and proposes to use that. Apparently he does not see that to calculate this ratio from the series he gives, it will be necessary to take a billion or so of terms! For he does not give any hint of any modification of the series, or the use of the value obtained for some small angle.

Lastly, with regard to the calculation, it is strange that the denominator chosen as a common denominator is 15 times what it need have been; also it is a matter of wonder, considering that tables of logarithms were known to Leibniz, as a reader of Mercator and

others, that Leibniz puts the matter in fractional form instead of working in decimals; thus, the arc whose tangent is 0.1 is equal to

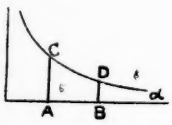
0.1	–	0.00033333333333333----
0.000002		0.00000001428571428----
0.0000000001111---		0.000000000000909----
0.100002000111111---		0.000333347619956---

$$=0.99966865249-----$$

Finally, note that while Wallis and Brouncker are mentioned, Barrow is not. This is all part and parcel of his successful attempt to conceal, from all but Oldenburg, the fact that he had a copy of Barrow in his possession, right from the commencement of his studies.

III.

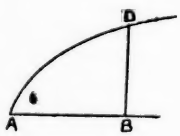
Transcribed from a manuscript tract of Newton on "Analysis by means of equations with an infinite number of terms."



AB $\sqcap x$, BD $\sqcap y$, a, b, c given quantities, m, n whole numbers. If then

$ax^{\frac{m}{n}} \sqcap y$, $\frac{an}{m+n}x^{\frac{m+n}{n}} \sqcap [\int y] \sqcap$ area of ABD.

In connection with this the following example is to be noted:

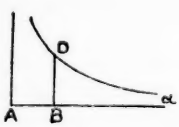


If $\frac{1}{x^2}$ $(\sqcap x^{-2}) \sqcap y$, that is to say, if $a=1$, $n=-1$, and $m=-2$, then we shall have

$$\left(\frac{1}{-1}x^{\frac{-1}{1}}\sqcap\right) -x^{-1}\left(\text{or } \frac{-1}{x}\right) \sqcap \text{aBD},$$

produced indefinitely in the direction of a; the calculation makes this negative because it lies on the other side of BD.

Again, if $\frac{1}{x}$ (or x^{-1}) $\sqcap y$, then $\frac{1}{0}x^{\frac{0}{1}} \sqcap \frac{1}{0}x^0 \sqcap \frac{1}{0}x^1$ (* this ought to be written $\frac{1}{0}$ 1*) $\sqcap \frac{1}{0} \sqcap$ infinity, which is the area of the hyperbola on either side.



If $\frac{1}{1+x^2} \sqcap y$, on division we obtain

$y \sqcap 1-x^2+x^4-x^6+$etc., and then

ABCD $\sqcap \frac{x}{1}-\frac{x^3}{3}+\frac{x^5}{5}-\frac{x^7}{7}+$ etc.; or, if the term x^2 is the first in the division, the value of y will be $x^{-2}-x^{-4}+x^{-6}-$etc.,

and hence BD $a \sqcap -\frac{x^{-1}}{1}+\frac{x^3}{3}-\frac{x^{-5}}{5}+$ etc.

The first method is to be used when x is small enough, and the second when x is large enough.

Gerhardt then remarks that Leibniz has noted completely the following two cases of extractions of roots:

$$(\surd a^2 + x^2) \sqcap y, \text{ and } \frac{\surd(1+axx)}{\surd(1-bxx)} \sqcap y.$$

Gerhardt further notifies the reader that he has omitted everything that he has found Leibniz to have copied out word for word, on comparison with Biot's edition of the *Commercium Epistolicum* (1856).

In the above, Leibniz marks interpolated remarks of his own with either [ ] or (* *).

In the same manner, Leibniz has written out word for word the part of the manuscript dealing with the solution of adfected equations (against this he has put the final observation: "And these things that have been given will be sufficient for the investigation of areas of curves"), in addition to the part which follows, "the application of what has been given to other problems of the same kind," which, as being already known to him, he has not copied out. He goes straight on to the next section, "To find the converse of the foregoing, that is, to find the base when given the area, and to find the base when given the length of the curve." He has written this out word for word; also he has noted fully to the end the "proof of the method of solution of adfected equations."

At the end of these extracts from Newton's tract follow the words, "I extracted these things from the letter of Newton 20 Aug. to Newton." Gerhardt states that he has already said all that is necessary about the contents of these extracts.

SECOND SHEET.

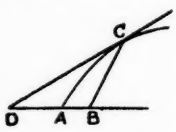
Extracts from the correspondence between Collins and Gregory.

Among a number of partly illegible and unintelligible notes the following were to be noticed.

Gregory, January, 1670: Barrow shows himself to be most subtle in the geometry of optics. I think that he is superior to all whose works I have looked into, and I esteem this author beyond anything that can be imagined.

Sept., 1670: I think that Barrow has gone infinitely further than all those who have written before him. From his method of drawing tangents, combined with certain meditations of my own, I found a general geometrical method of drawing tangents, without calculation, to all curves, which not only contain his particular

methods, but the general method as well. This is shown in 12 propositions.



Letter of Newton, 1672: ABC is any angle, AB $\sqcap x$, BC $\sqcap y$. Take, for example, the equation,

$$\overset{3}{x^3} \overset{2}{-2x^2y} + \overset{2}{bx^2} \overset{1}{-b^2x} \overset{0}{-by^2} \overset{0}{-y^3} \sqcap 0.$$

Multiply the equation by an arithmetical progression, both for the second dimension y and for x; the first product will be the numerator, and the other divided by x will be the denominator of a fraction which will express BD, thus.

$$\text{BD} \sqcap \frac{-2x^2y + 2by^2 - 3y^3}{3x^2 - 4xy + 2bx - b^2}.$$

Moreover that this is only a corollary or a case of a general method for both mechanical and geometrical lines, whether the curve is referred to a straight line, or to another curve, without the trouble of calculation, and other abstruse problems about curves, etc. This method differs from that of Hudde and also from that of Sluse, in that it is not necessary to eliminate irrationals.

NOTE.

It is almost useless trying to write a critical note on the above in such an incomplete state. But I may remark that Leibniz apparently was at the time quite ignorant of what we now term "putting in the limits for a *definite* integral."

Gerhardt considers that the existence of this extract proves conclusively that Leibniz did not see the letter of Newton so often referred to; forgetting, as Sloman remarks, that Leibniz ought not to have seen the tract at all!

P. S. In allusion to footnotes 3 and 18, with regard to the use of the word "moment" or "momentum" in the sense used by Leibniz, I have found (since the above was written) that Cavalieri, in his *Exercitationes Sex*, defines the term in the mechanical sense and gives much of the matter of Pascal on Centers of Gravity, as it appears in the "Letters of Dettonville." I suggest that Leibniz saw it in Cavalieri, and that its origin is to be traced to Galileo. J. M. C.

OUR MUSICAL IDIOM.

WITH AN INTRODUCTION BY GLENN DILLARD GUNN.

INTRODUCTION.

The effort to expand the means of musical expression is as old as the art itself. It is recorded in each chapter of musical history; it has been interrupted only during those periods of the art's development wherein the composer has been concerned with the completion of art-types already defined.

Every advance in the art has been prefaced by a period of experimental effort, which has sought new modes of expression. So soon as these modes of expression have been defined and their tendencies and the laws governing them have been apprehended, experiment has been replaced by careful conformance to law and tradition, which has operated to the perfection of the new art type.

The present is preeminently an epoch of experimentation. The old art types have been completed. The harmonic vocabulary based upon the sequences of tonality established in these completed art forms also has been exhausted and for the past half century composers have been concerned with the development of new harmonic idioms. (Viz., Liszt, Wagner, Strauss, Franck, D'Indy, Debussy, Ravel, Shoenberg, Busoni.) As these composers have discovered and employed new harmonies, new scales and new sequences of tonality with their resultant new harmonic progressions, the theoretician has endeavored to classify their discoveries according to his established system with results weirdly confusing. The crying need of the moment seems to be a new system for the naming and classifying of all possible tonal combinations.

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"Harmony is that which sounds together," wrote Bernard Ziehn twenty-five years ago. But the average theoretician comprehends only those simultaneously produced sounds which may be arranged in series of superimposed thirds. In the meantime the composer has consciously employed many harmonies which are not formed of superimposed thirds. (Viz., Debussy's major second as the first interval of the tonic chord, or Shoenberg's combinations of superimposed fourths, to cite familiar examples.) The executive artist, upon whom the composer is dependent for the delivery of his message, is, in turn, dependent upon the theoretician for a logical classification of the new harmonies. The composer of the present is almost equally dependent upon some scientific classification of his material. Naturally the public has first looked to him for this classification. But he seems able to make it only for himself, as Reger and Shoenberg have done. In any event the world has been slow to adopt these special classifications and is still seeking a general system that will include all possible harmonies in logical order.

That system has been evolved by Mr. Ernst Lecher Bacon of Chicago, who by applying the principles of algebraic permutations to the problem has succeeded in formulating all harmonies that may possibly exist in the present system of twelve tones (of itself a most important service) and having formulated them, has found a system of nomenclature which actually describes any possible combination of tones and makes a general or special classification possible.

The value of this new system of nomenclature to the executive artist is immediately apparent. That puzzled individual may name and classify the new tonal combinations which he is required to memorize and present convincingly to the public. The composer is even more importantly served by Mr. Bacon's researches. For he is shown at a glance all possible harmonies (there are but 350) and all possible scales (of which there are about 1490). He may select from the clear and concise tables placed at his disposal those harmonies and scales which seem to him useful and beautiful and having familiarized himself with their color and feeling, in short, made them a part of his own consciousness, may employ them subjectively to the expression of feeling and sensibility, to the building up of his own especial harmonic idiom. For though the composer's work is best when it is most subjective, he is constantly obliged to concern himself with the facts of his art and out of these facts to fashion

that delicate fabric of feeling and fantasy which is to give freer and fuller powers of expression to the music of the future.

GLENN DILLARD GUNN.

THE FORMATION OF SCALES.

The chromatic scale has become established as the basis of modern harmony. Though the major and minor modes are still accorded that recognition which the printed key signature seems to imply, modern compositions so bristle with accidentals that even to the eye, and still more to the listening ear, is it evident that the restrictions of the major and minor system have been destroyed.

However, few of our modern composers treat the chromatic scale purely in itself; the intrusion of other scales which are synthetically formed from the chromatic scale is always felt. The chromatic scale is only the analytic product of others, which consist of combinations of its semitones, wherein intervals are found which are collections of semitones. Of these synthetically formed scales there are many in number but few in use; and each may form a separate harmonic basis. Beethoven and Liszt, the latter more notably, occasionally used scales differing markedly from the major and minor; but their appearance was only incidental, and the scales were rarely made use of as bases for harmonic systems. Busoni created, as he says, 113 different scales through rearrangements or permutations of the intervals of the major and minor scales.[1] Debussy has used a few unfamiliar scales, notably the whole tone scale, for a thorough harmonic basis. However, as will be shown, a vast number of scales that have never before been conceived are opened to discovery through the application of the principles of algebraic permutation to arrangements of tonal sequences.

A *scale* is a series of ascending or descending tones. Such a series may conform to a pattern, a regularly recurring succession of intervals in certain order, bounded by a fixed interval; or it may not conform to pattern. The pattern may or may not conform to the duodecimal system. If the scale conform to pattern it must be bounded by a fixed interval. Intervals of simple physical ratio are preferred, and these are found, slightly tempered, in the duodecimal system.

Until now, the octave only has been consciously used as a fixed

[1] His figures are incorrect as, mathematically computed, the number of permutations of the combination of intervals is: (*a*) of the major scale, 21; (*b*) of the minor scale, 140.

interval, but there is no reason why, in specialized cases, other intervals could not exist between corresponding tones in recurrences of the pattern. We may have scales repeating at each fourth, fifth, sixth, seventh, ninth, tenth, etc. But because we are at present engaged in a classification of scales which incidentally involves the discovery of a multitude of unheard-of ones, and because a classification of such scales as these would be of formidable length, we must be content to study that most important class of scales in which each succeeding repetition begins an octave above or below the preceding one.

Again we must distinguish between two classes of scales whose basis is the octave. The first class is that one in which the smallest scale-units in the octave number 12; this is our *duodecimal* system. The second class contains many systems, in each of which the number of the smallest units is either greater or less than 12. We will consider both of these classes, for in the consideration of the first class we can enlarge considerably the present scope of the duodecimal system, while in the consideration of the second class we may discern dimly certain possibilities of the future. First we will discuss the scale possibilities of the duodecimal system.

By a division of the octave into twelve parts the common *chromatic* scale is formed. Now by grouping together certain of these twelfths of an octave, the so-called "semitones," we may form scales whose gradation is uneven and less refined than that of the chromatic scale. If we are given a certain combination of intervals which, added together, give the octave, we can permutate these in a number of different ways; that is, we can rearrange the given intervals to form different scales. We also may have combinations in which the same intervals occur more than once. If n is the number of intervals between octaves in the scale and n_1 of them are alike, and n_2 others are alike, etc., the number of scales (P) that can be formed by permuting the given combination of intervals is:

$$P = n!/n_1!n_2!\ldots.$$

(The exclamation point, read "factorial," denotes that the number which it follows is a product of all integers less than and including itself, each integer being a factor only once.)

For example, we desire to find the number of scales that can be formed with the intervals of the major scale. The major scale consists of 5 whole tones and 2 half tones, making a total of 7 intervals.

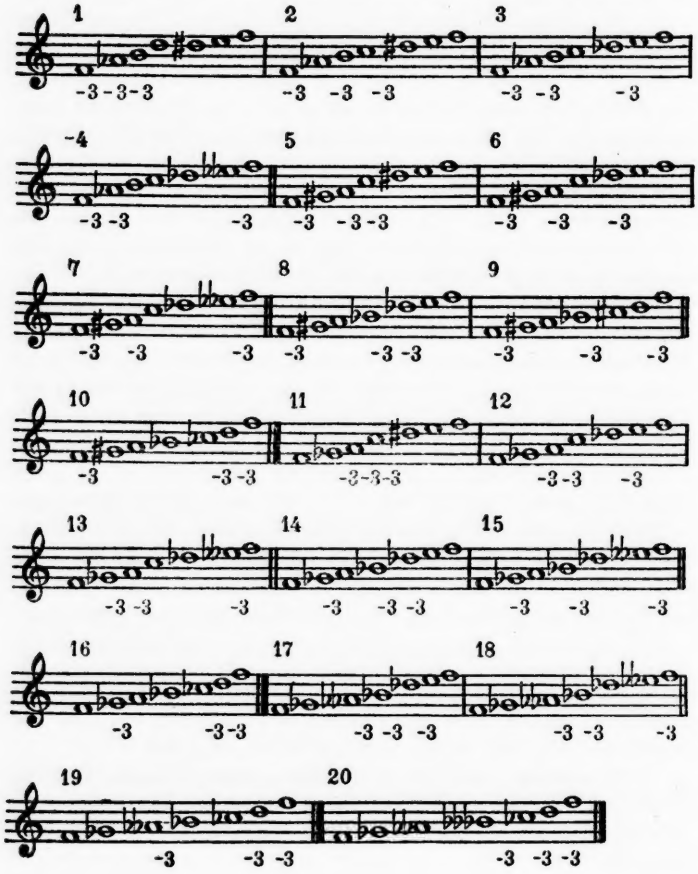
Thus $P = (n!/n_1!\,n_2!) = (7!/5!.2!)$

$= [1.2.3.4.5.6.7/(1.2.3.4.5.)\,(1.2)]$

$P = 42/2 = 21.$

As an example of the way in which all possible scales can be formed out of a certain combination of intervals, 20 scales will be formed out of the combination (three minor thirds and three minor seconds) as follows:

The symbol (— 3) will be written below respective minor thirds.



It will be observed that a new series begins with each double or triple bar.

A triple bar is written before each chromatic elevation of the lowest minor third.

A double bar is written before each chromatic elevation of the middle minor third.

The uppermost minor third always starts at its lowest possible position and is raised successively to its highest possible one, after which a change is made in the relative position of the lower minor thirds.

This method of forming all scales from a certain combination of intervals is purely arbitrary.

Now it is possible to form a great number of combinations in which the sum of the intervals is an octave. Moreover, as we have seen, usually a number of scales can be formed out of each combination. Each scale is to be considered a permutation of the combination's intervals.

In the following table will be found every combination possible with intervals as small as the minor second and not greater than the major third. Intervals larger than the major third are not used because in the formation of scales they would make gradation too abrupt and uneven. The table will also include calculations of the number of permutations (to be regarded as the number of scales) possible with each respective combination, according to the formula. The vertical columns contain the intervals minor 2d, major 2d, minor 3d, major 3d, respectively. The horizontal rows of numbers are the combinations. A number (n) falling in a vertical column (v) means that the interval (v) is repeated n times in the combination in which n lies (see Table I).

To make the function and construction of the table more plain two of the combinations may be explained. Combination 1 indicated by the number in the extreme left-hand column, consists of twelve semitones. It is therefore a formula of the chromatic scale, and has therefore only one permutation. Combination 21 contains two minor seconds, two major seconds and two minor thirds. From it may be formed fifteen scales or permutations.

Means have now been shown to find all possible scales in the twelve-tone system, scales which have intervals exceeding the major third in size being omitted. Adding the number of permutations formed with all combinations a total of 1490 scales is found.

A systematic study of these 1490 new scales would lead to the discovery of many valuable scales. I have found many that are interesting by this method, but will mention only a certain class of these scales, which I will call *equipartite* for want of a better name.

TABLE I.

	Combinations				Permutations			Combinations				Permutations	
	Minor Seconds	Major Seconds	Minor Thirds	Major Thirds	Calculations	Permutations		Minor Seconds	Major Seconds	Minor Thirds	Major Thirds	Calculations	Permutations
1	12				P12!/12!	1	18	3		3		6!/3! 3!	20
2	10	1			11!/10!	11	19	2	5			7!/2! 5!	21
3	9		1		10!/9!	10	20	2	3		1	6!/2! 3!	60
4	8	2			10!/2! 8!	45	21	2	2	2		6!/2! 2! 2!	90
5	8			1	9!/8!	9	22	2	1		2	5!/2! 2!	30
6	7	1	1		9!/7!	72	23	2		2	1	5!/2! 2!	30
7	6	3			9!/3! 6!	84	24	1	4	1		6!/4!	30
8	6	1		1	8!/6!	56	25	1	2	1	1	5!/2!	60
9	6		2		8!/2! 6!	28	26	1	1	3		5!/3!	20
10	5	2	1		8!/2! 5!	168	27	1		1	2	4!/2!	12
11	5		1	1	7!/5!	42	28		6			6!/6!	1
12	4	4			8!/4! 4!	70	29		4		1	5!/4!	5
13	4	2		1	7!/2! 4!	105	30		3	2		5!/2! 3!	10
14	4	1	2		7!/2! 4!	105	31		2		2	4!/2! 2!	6
15	4			2	6!/2! 4!	15	32		1	2	1	4!/2!	12
16	3	3	1		7!/3! 3!	140	33			4		4!/4!	1
17	3	1	1	1	6!/3!	120	34				3	3!/3!	1
												Total	1490

An *equipartite* scale is one in which the same pattern of intervals is repeated an integral number of times within the octave. If a scale is *bipartite* a group of intervals will appear twice within the octave with no remainder; if the scale is to begin on F its two parts begin, respectively, on F and B. As a result in this case it is immaterial whether the tonic is B or F, for the scale sounds alike either way, except for the transposition.

We may split the sum of twelve semitones (semitones being regarded as intervals) into two parts or three. Dividing it into two parts, each part containing six semitones, allows us again to divide this semi-octave into two or three parts. Dividing the octave into

TABLE II (FOR BIPARTITE SCALES).

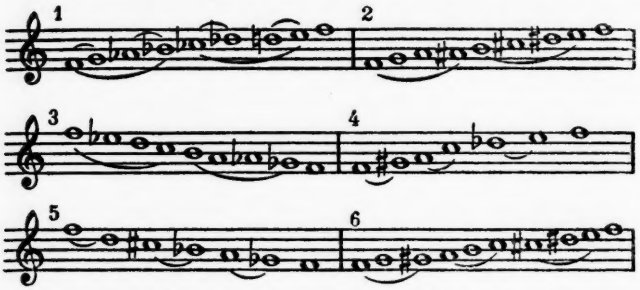
	Combinations				Permutations	
No.	1 Minor Second	2 Major Second	3 Minor Third	4 Major Third	Calculations	No. of Perm.
1	6				P=6!/6!	1
2	4	1			P=5!/4!	5
3	3		1		P=4!/3!	4
4	2	2			P=4!/2! 2!	6
5	2			1	P=3!/2!	3
6	1	1	1		P=3!	6
7		3			P=3!/3!	1
8		1		1	P=2!	2
9			2		P=2!/2!	1
					Total	29

TABLE III (FOR TRIPARTITE SCALES).

	Combinations				Permutations	
No.	1 Minor Second	2 Major Second	3 Minor Third	4 Major Third	Calculations	No. of Perm.
1	4				P=4!/4!	1
2	2	1			P=3!/2!	3
3	1		1		P=2!	2
4		2			P=2!/2!	1
5				1	P=1!	1
					Total	8

three parts, each part has four semitones, which may again be divided by two. Thus we may split the octave into 2, 3, 4, and 6 equal parts. Scales formed by such divisions may be called, respectively, bipartite, tripartite, quadripartite, and sexpartite. As the

last two types may be classed under the first and second they do not require a separate classification. In Tables II and III the combinations in the bipartite and tripartite types are given; in other words, the possibilities of combinations with six and four semitones, respectively, are shown. Each arrangement of a combination is then repeated in the remaining half or two-thirds of the octave.

A few interesting equipartite scales are herewith shown:

(1, 2 and 3 are from Table 2, combination No. 4; 4, 5 and 6 are from Table 3, combination No. 3.)

Scales formed by permutating combination No. 4 in Table II, and combinations Nos. 2 and 3 in Table III are especially interesting. No. 1 is formed by alternating major and minor seconds, while No. 3 is formed in the same way, except that in it the order of the intervals of No. 1 is reversed. Even such a mechanically formed scale as this sounds beautiful and original. It is a noteworthy fact that in scales 1 and 3 the chords formed on every degree are diminished. Scales Nos. 4 and 5 are built similarly; only a minor third and a minor second alternate. Chords formed on every degree of these scales are augmented.

SCALES FORMED FROM SYSTEMS OTHER THAN THE DUODECIMAL.

Although to-day the importance of systems containing other intervals than multiples of semitones is questionable, it is nevertheless interesting to know that such systems may be exploited for scale and harmonic possibilities in the same manner as our present system. Busoni has already experimented with the *tripartite* tone scale; that is, a scale in which each whole tone is divided into three instead of two whole parts. The physicist may scorn the idea of a

new system, knowing that the duodecimal system contains the simplest physical intervals, yet it must be remembered that the perfect intervals are also not found in the 12-tone system, because of "tempering." Moreover in the other systems many of the most important intervals of the duodecimal system will be duplicated. Although probably no system will ever be of equal importance with the duodecimal, it is not inconceivable that, just as certain new scales within our present system have been chosen by recent composers as harmonic and melodic idioms of expression, so certain "foreign" systems may once be chosen for similar purposes.

Accordingly, we are to consider any equal divisions of the octave. However, certain divisions, as for example into 11 or 13 equal parts, are not of importance, since the intervals formed in this way would only be confounded with poorly tuned intervals of the 12-tone scale. In order to discriminate in the selection of numbers with which to divide the octave it is well to choose only those numbers which are multiples of the smallest prime numbers, 2, 3, and 5. We may call each of these systems an "N-tone chromatic system." If the system is one in which the number of smallest intervals is 9, we may call it a 9-tone chromatic system. We are not bound to confine the use of the term "chromatic" to our duodecimal system, since in its musical application the word is used to describe a succession of the smallest possible intervals.

In considering the N-tone chromatic systems we may go through the same steps through which we have passed in considering the duodecimal system. In each of these unfamiliar systems there are chromatic intervals which may be combined and permutated to form scales of more rapid and uneven gradation. Just as before, we have to set a certain limit to the size of an interval employed in one of these scales. In the five-tone system a coupling of only two chromatic intervals produces an interval almost too great to exist in a scale of moderately refined gradation. In the 24-tonal system a coupling of as many as 6 intervals into 1 is acceptable. It will readily be seen that to construct tables for all N-tonal systems through which an infinite number of gradations is possible, would require much space. It has already been stated that those systems having numbers of chromatic intervals equal to multiples of 2, 3 and 5, are most important. They are systems of 4, 5, 6, 8, 9, 10, 12, 14, 15, 16, 18, 20, 21, 22, 24, etc., chromatic tones; for demonstration I will select only 2 of these; namely 8- and 9-tone systems.

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In the tables that follow I will give the number of combinations and calculate the number of permutations for each combination with the selected N-tonal systems. In other words, scale possibilities with systems having 8 and 9 tones will be shown in each respective table.

TABLE II, N=8

	Combinations			Permutations	
	1/8 Octave	2/8 Octave	3/8 Octave	Calculations	Permutations
1	8			8!/8!	1
2		1		7!/6!	7
3	5		1	6!/5!	6
4	4	2		6!/2! 4!	15
5	3	1	1	5!/3!	20
6	2	3		5!/2! 3!	10
7	2		2	4!/2! 2!	6
8	1	2	1	4!/2!	12
9		4		4!/4!	1
10		1	2	3!/2!	3
				Total	81

Intervals used do not exceed 3/8 octave.
Number of intervals corresponding to Duodecimal System=4.

TABLE III, N=9

	Combinations			Permutations	
	1/9 Octave	2/9 Octave	3/9 Octave	Calculations	Permutations
1	9			9!/9!	1
2	7	1		8!/7!	8
3	6		1	7!/6!	7
4	5	2		7!/2! 5!	21
5	4	1	1	6!/4!	30
6	3	3		6!/3! 3!	20
7	3		2	5!/2! 3!	10
8	2	2	1	5!/2! 2!	30
9	1	4		5!/4!	5
10	1	1	2	4!/2!	12
11		3	1	4!/3!	4
12			3	3!/3!	1
				Total	149

Intervals used do not exceed 3/9 octave or a major third, as translated.
Number of intervals corresponding to Duodecimal System=3.

The Numbers of Tones and Intervals Found Correspondingly in Any Two N-Tonal Systems.

If we choose a common tonic for all N-tone chromatic scales we will find certain other tones which are common to two or more of these scales. For example, if we form both a 9-tone chromatic and a duodecimal scale upon C, we will expect to find two tones in common besides the C and its octave. They will be E and G sharp; for each of these tones marks the partition of the octave into three equal parts. This means that certain intervals in one system are

the same as intervals of another. But an interval common to two systems cannot be the same multiple of the smallest unit in each system. If we desire to find the number of intervals which are found correspondingly in each of the two systems, we need merely to find the largest factor common to the number of chromatic divisions of both systems. For example, to find the number of intervals which are common to the 18-tone and the 12-tone chromatic scales we find the G. C. F. of 18 and 12, which is 6. This is the desired number. Of course, intervals which are multiples of this common interval (the whole-tone, in this case) are also common to both systems.

Intervals of N-Tone Chromatic Scales.

Throughout our entire treatment of scale possibilities there is one interval which remains constant; namely, the octave. The ratio of this interval, that is the ratio[2] of the vibration frequency of the higher tone to that of the lower tone is always 2. If N is the frequency of the lower tone, its octave is 2 N. Now N and 2 N may be written as 2^0 N and 2^1 N respectively, since any quantity with an exponent 0 equals unity. It is evident that the frequencies of any tones between $N \times 2^0$ and $N \times 2^1$ can be expressed as N times the coefficient 2 with an exponent varying between 0 and 1.

If the octave contains r equal intervals, the difference between 0 and 1 of the exponent of 2 will be divided into r parts. This is true because (*a*) equal intervals form equal ratios of vibration; and (*b*) equal ratios may be expressed as the quotients of a constant in which the difference of the constant's exponents in the numerators and respective denominators remains constant. To illustrate:

$$2^1/2^0 = 2^{1-0} = 2$$
$$2^6/2^5 = 2^{6-5} = 2.$$

Hence $2^1/2^0 = 2^6/2^5$.

Thus

$$2^{\frac{1}{r}} \text{ or } \sqrt[r]{2}$$

expresses the ratio of any interval formed by two adjacent tones in an equally tempered scale of r intervals. Moreover the intervals which any tonic (arbitrarily chosen in the case of the equally tempered scales) forms with the successive ascending tones above it, are, respectively:

$$2^{1/r}, 2^{2/r}, 2^{3/r}, 2^{4/r}, \ldots\ldots\ldots 2^{(r-1)/r}, 2^{r/r} \text{ or } 2.$$

[2] This ratio is physically defined as the interval itself.

From these facts we derive two general formulas: (A) expressing the physical interval or vibration ratio between 2 tones and (B) the vibration frequency of any tone lying above a given tone, N.

(A) $\quad I\,(\text{interval}) = 2^{c/r},$

(B) $\quad V\,(\text{vib. freq.}) = N.2^{c/r},$

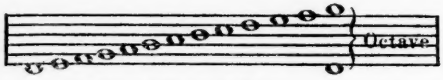
where r = the number of chromatic intervals per octave, in the given system; and c = the number of chromatic intervals separating the two tones whose physical ratio is to be found.

With these formulas we can express the various intervals of any equally tempered scale.

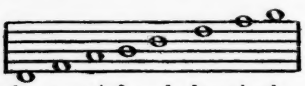
NOTATION OF SCALES.

In considering the great number of scales of which we have learned in the previous section we are confronted with the problem of their notation. Our present notation is really suited for seven scales only; namely, the major scale and the scales formed by cyclically rotating the permutation of the intervals of the major scale, that is, the Dorian, Phrygian, Lydian, Mixolydian, etc. We cannot write even a minor scale without the use of an accidental. Then with regard to the 1483 other scales, because of this great number and variety, we cannot do more than make general statements.

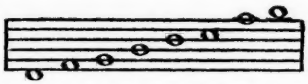
We realize, to begin with, that the ideal notation of our present-day music should be one which is designed to eliminate the inconveniences of accidentals. Such a notation would be naturally one designed from the chromatic scale; and because the chromatic scale contains all of the 1490 other scales of the duodecimal system, it would be adaptable, in a perfect sense, to all of these scales. We could accomplish the notation of the chromatic 12-tone scale with a six-line staff giving each degree a separate line or space, as shown:



The major scale on this staff would be:



The minor scale would be:



The mental picture we obtain of the relation of the intervals of these two scales in this manner is alone an advantage. Furthermore, in the six-line staff notation we are less bound to avoid deviations from our chosen scale; we are freer to escape from the tyranny of sharps and flats. An abhorrence of accidentals has always tied us to our chosen scales. Other advantages of this notation could be cited, but the chief one is, of course, that merely through the addition of another line (which does not confuse us optically) we are able entirely to avoid accidentals.

However, the difficulty of introducing this system into common use would be almost too great to be overcome. An attempt at this could be likened to the recent attempts at introducing a universal language; for were we all to learn a universal language we would still have to retain a knowledge of the old for its literature. We are therefore compelled to adjust our new scales to the common notation of the five-line staff.

We may eliminate from consideration not only the major scale and those scales formed by a *cyclic rotation*[3] of its permutation of intervals, but also the minor scale with its corresponding scales formed by a similar cyclic rotation. This suggests to us a process that will greatly simplify our whole problem. We see that the notation for one scale is suitable for all other scales formed by a cyclic rotation of the permutation of its intervals. The number of these scales will depend upon the number of tones or intervals in the original one. The notation for a scale of n tones or n intervals will serve for $(n-1)$ cyclically related scales. Thus one notation serves for n scales.

We realize that out of a certain combination of intervals we may form more than one cyclic group, for some combinations have as many as 168 scales while in no cyclic group can there be more than 11 different scales. A formula with which we may calculate the number of cyclic groups in each combination is:

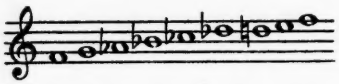
$$G=(n-1)\,!/n_1\,!\,n_2\,!\,n_3\,!\ldots.$$

where n is the number of intervals in a combination, and n_1, n_2, n_3,

[3] The term defines itself. A *cyclic rotation* of a permutation is one in which the terms are always written in the same order, but each successive permutation begins with the second term of the preceding one. The following is a *cyclic* group of permutations: A B C D, B C D A, C D A B, D A B C.

etc. are the numbers of times respectively which certain intervals are repeated in the combination. There are few exceptions to this formula, all of which are of one type. The erroneous type is that in which $(n_1+n_2+n_3+n_4\ldots)$ exceeds $(n-1)$. These exceptions often cause fractions which cannot be integrally expressed. In cases of this exception we must find our number of cyclic groups by actual trial. But if we have found one signature suitable for each whole cyclic group of scales we have, in general, shown only one-twelfth of possible signatures, for in most cases a different signature is necessary for each chromatic degree. Only in equipartite scales are fewer signatures than twelve necessary to each group. If a scale is bipartite only six signatures are necessary; if tripartite, four; if quadripartite, three; and if sexpartite, two.

As we are considering these 227 scales representing cyclic groups primarily for their notation, we are confronted with the question, what signature shall we give to a work based on a scale like the following?



None of our conventional signatures for major scales will apply to this scale; for we see the three essential signatures are:

d flat being unnecessary as a signature because it is cancelled immediately, the scale being an 8-tone scale, which necessitates the repetition of one note.

We will find that most of the scales, like this one, will require signatures other than those which we have employed for our major and minor modes. Consequently we will not try to reconcile our customary signatures with those natural to the new scales. Therefore, in order to make a signature for any scale on any degree, write down those accidentals which appear in the notation of the scale, omitting those accidentals only which are cancelled as the scale continues. We may rightly call this a *natural* system of signatures.

Concerning the method of finding each scale representative of a cyclic group for a given scale degree, the following means are perhaps the simplest:

1. Choose an interval which occurs singly in the combination and place it in the lowest position in the scale.

2. Permutate the other intervals above it in every possible way.

3. Each permutation, with the first interval remaining in a fixed position, will form a desired scale.

4. When no interval occurs singly in the combination there is no rule which applies generally; but because of the small number of combinations of this character the desired scales can be easily found by trial.

There is little need for investigating the problems of notation of N-tonal systems until such systems come into use. Solutions to such problems are really simple and arbitrary. Suffice it to say, there is no need of retaining the five-line staff for N-tonal notation. It would be unfortunate if one were compelled to read a totally new system of intervals from a staff with which one would constantly associate accustomed intervals.

Although it may seem strange that so much attention is paid a subject like the formation of scales, there is nevertheless justification in an investigation of this sort. A scale has far greater importance than the mere sequence of tones comprising it would imply. Practically all of the hundreds of melodies we know can be formed, almost without accidentals, from the major and minor scales. Virtually all of the common harmonies can be constructed from these modes. The vast amount of musical thought and feeling has until recently expressed itself in major and minor. But the chromatic scale offers a much wider field of expression; for it contains not only the major and minor scales, but over fourteen hundred others. Nevertheless, although the chromatic scale has become the basis for modern harmony, melody does not seem to flow freely chromatically. Our musical speech continually demands some simple group of tones and larger intervals. Without some limitation more binding than the chromatic scale, we are helplessly confused with the wealth of possibilities. Such limitations are found among the multitude of scales derived synthetically from the chromatic.

Debussy and some of his colleagues have made their idiom or "dialect," as it were, the whole-tone scale. This one scale, because of its uncertain "tonality," and its "color," has been the outstanding characteristic of the French impressionists.

A few other scales, such as the Greek "modes," which are all cyclically related to the major scale, have been the basis for numer-

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ous works. On the whole, there is no reason why other scales, among the vast number shown to exist, should not become equally important idioms of expression.

Limitations of space unfortunately prevent me from tabulating completely the fourteen hundred and ninety scales of the duodecimal system.

Concerning the N-tone scales, it is well to consider for illustrative purposes the words of Busoni in regard to his tripartite tone scale:[4] "The tripartite tone," says he, "has for some time been demanding admittance, and we have left the call unheeded." With the tripartite tone he encounters a difficulty which will be found also in considering other N-tone scales. He says we would lose through the tripartite tone the minor third and the perfect fifth. Now a chromatic scale in which the most important intervals do not occur (intervals whose ratios are expressed as quotients of the smaller integers) will never form quite as valuable a system as a chromatic scale that contains them. Realizing this, Busoni has attempted to reconcile the 12-tone with the 18-tone system; that is, a system of bipartite tones with one of tripartite tones. His solution is naturally a 36-tone scale involving the sexpartite tone. To entertain any hopes for a system of sexpartite tones seems to me futile. A system of 24-tone chromatics might be better reconciled with our duodecimal system. This example merely shows us that we cannot attempt to reconcile the N-tonal systems with each other or with the duodecimal system. An 18-tone chromatic system is perhaps next in importance to the duodecimal system, but it is comprehensive and important enough in itself, even though it does not contain minor thirds and perfect fifths.

Again we must consider how we are to produce these tones, as Busoni has mentioned in regard to his tripartite tone scale. For experiment and a training of the ear to the tripartite tone Busoni recommends Dr. Thaddeus Cahill's dynamophone, an instrument which would, however, be very difficult to obtain or to construct. A Seebeck's siren with a special disk for each system would be a good substitute. The number of holes in each circular row could be mathematically computed with the help of the formulas:

$$I = 2^{d/r} \text{ and}$$
$$V = N.2^{d/r},$$

[4] For Busoni's statements read his *Sketch of a New Esthetic of Music*, New York, 1911.

which are explained in previous pages. A motor to revolve the disk would furnish a constant speed of rotation.

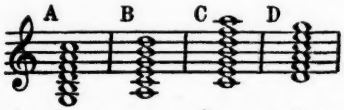
Such experiments would furnish means of acquiring a sense of intervals other than those to which we are accustomed; but, in Busoni's words, "only a long and careful series of experiments and a continued training of the ear can render this material approachable and plastic for the coming generation, and for art."

A NEW HARMONY.

Our present system of harmony, the system of chords (harmonies formed of superimposed thirds), is deficient in two important respects. First, it is often unwieldy; and second, it is not fully comprehensive. This latter shortcoming is partly responsible for the former, since it is true that we may represent certain harmonies, seemingly not within the scope of our system, in complex ways. To illustrate: let us examine the various unsatisfactory ways of describing the simple harmony:

If the harmony is a chord, we must be able to build it up by superimposing thirds. But no complete chord exists that contains each of these and only four notes.

But there are chords containing more than four notes which contain the notes of the harmony, such as the following:



From these chords we may strike out those notes foreign to the harmony and derive what we call an *incomplete* chord. The harmony:

may therefore be termed an *incomplete 11th chord* (as in A, B, or D) or an *incomplete 13th chord* (as in C). If we are willing to recognize an incompleteness of this sort as mathematically rigid, we must still admit that such a naming of the harmony as an incomplete 11th does nothing more than justify its existence among chords. It does not name the harmony for there are innumerable

incomplete 11ths. To name the 11th chord in each case is difficult, the general method being that of determining upon what degree of the major or minor scale it is built. But, again, must we consider all harmonies in the light of the major or minor scales to-day when many other scales are being used? Furthermore we must find where the incompleteness lies. Lastly one would suppose that every harmony has one fundamental position, but here are four. One should be able to tell what sort of inversion of the fundamental harmony the one in question is. How is this possible when the harmony in its position is a different inversion with each fundamental?

If we allow ourselves the latitude of recognizing diminished thirds, we may say the harmony is composed of two diminished thirds separated by a minor third. Taking this liberty we might have a *specialized chord* or so-called *altered chord,* but how shall we describe any particular one? Moreover, it is false to assume that diminished thirds are thirds at all; for they are seconds.

Sometimes, if the harmony is preceded or followed by others, we may analyze it under our present system by considering certain tones as "passing tones," "suspensions," "afterbeats," "syncops," "organ-point," etc. The awkward system of figured bases sometimes affords a means for expressing simpler chords.

If such is the fate that a simple harmony like the above suffers in analysis, what lot befalls the multitiude of more complex harmonies? The best that modern analysis can do for them is to treat them in relation to surrounding harmonies. Even then, "unresolved suspensions," etc. are continually met with in modern music. If harmony is "that which sounds together," we should be able to define any combination of simultaneously sounding tones, whether this combination is surrounded by others or not. A note suspended from a consonant to a dissonant chord is sounding in the second as well as in the first harmony. Does not an organ-point form a separate harmony with each of a series of chords "moving through it," even though these chords are dissonant with the organ-point? A harmony is a harmony whether dissonant or consonant. Yet of the vast majority of dissonant harmonies few can be adequately named and classified in themselves.

The chord system is adequate in analysis of older works only. It can give only a superficial analysis of modern works.

A more important objection even than that of inadequate nomenclature is that by reason of our use of the chord system we

are hindered in enlarging our scope of harmonies. The conception of harmonies given us by this system restrains us from enlarging our harmonic vocabulary. Bred in the chord system, we are prone to regard any harmony which is not chordic in construction as a mere variance of some "simple" chordic form. Many a stereotyped theorist would shudder at the notion of giving the above harmony a prolonged and separate existence. It must be immediately resolved into a stable form; the tonic triad of G major, etc., etc. Are we blind to the existence of harmonies not made up of superimposed thirds? Shall we refuse to recognize non-chordic harmonies merely for the technical reason that we employ a system of superimposed thirds, which was an expedient solution to theoretical problems two centuries ago? Because of the limitations of our present system, a vast number of harmonies remain to-day virtually undiscovered. Although many have been employed passingly and subconsciously, few have been employed deliberately, few are spoken of as a part of the composer's vocabulary.

Fully realizing the importance of the chord system in the analysis of older works (for these were conceived in the spirit of the chord system), I believe it is important that a new and fully comprehensive system should supplement it, a system that would prove adequate for the analysis of modern writings. Whereas the old system embraces chords only, the new should embrace all harmonies. The chord scheme would then take its place as a sub-system of the more general and all-inclusive system.

Just as the present method is more than a mere scheme of nomenclature, so the one which I propose should be considered as affecting more than the mere naming of harmonies. The chord system teaches us that all harmonies are chords, are built by imposing major and minor thirds upon each other. The proposed system should, as will be shown, teach us to recognize harmonies which are built by superimposing any intervals. It should teach us a broader conception of harmonies and, as I believe, a more valuable one, since the importance of a notion usually depends upon its generality.

To-day the modern composer habitually employs the twelve-tone scale as the source of his harmonic invention. The abundance of accidentals in our modern composition is superficial but none the less accurate evidence of the passing of the feeling for the diatonic modes. To-day there are also a few scales which are formed of new arrangements in the intervals of our duodecimal system.

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But the octave still remains the common basis for all scales now used; each scale repeats itself at successive octaves.

It seems only natural that we make this interval which is of the greatest importance because it has the simplest ratio, the basis for our harmony. We may therefore call its interval unity.[5]

Having established the octave as the basic interval, and having assigned to it the number one, we turn our attention to the lesser intervals. The semitone, since it is one-twelfth the gradation of the octave, will be known as the interval, one-twelfth. The "whole tone" becomes two-twelfths. Tabulating all of our intervals in their old and new nomenclature we have:

DIATONIC NAME	NATURAL OR CHROMATIC NAME
Minor Second	One Twelfth
Major Second	Two Twelfths
Minor Third	Three "
Major Third	Four "
Perfect Fourth	Five "
Augmented Fourth	Six "
Perfect Fifth	Seven "
Minor Sixth	Eight "
Major Sixth	Nine "
Minor Seventh	Ten "
Major Seventh	Eleven "
Octave	One.

Although many of these fractions expressing intervals are not reduced to their simplest form, it is of advantage to retain the common denominator, twelve; for if all intervals can be expressed as quotients of variable integers and the constant twelve, we need consider only the numerators and eliminate the common denominator. Thus the intervals, one-twelfth, two-twelfths, three-twelfths etc., may be called respectively, one, two, three, etc. It is clear that this nomenclature is founded entirely upon the chromatic scale since every interval is measured as a multiple of the intervals comprising the chromatic scale.

In naming harmonies having more than one interval, the advantages of the chromatic nomenclature are immediately apparent. For instance, the major triad is said to be formed of a minor third placed above a major third. In other words the interval 3 is placed above the interval 4. Thus the major triad in fundamental position

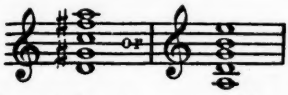
[5] For simplicity we call the interval unity, although the physical interval of the octave is 2.

is a $^{3}_{4}$ or 4-3 harmony. Likewise, the minor triad is a 3-4 harmony; the dominant sept chord in fundamental position, a 4-3-3 harmony; and the dominant sept chord in its first inversion is a 3-3-2 harmony. The harmony under previous discussion is, in the form

a 2-3-2 harmony.

This change in nomenclature means more than is at first apparent. It means an expansion of our conception of harmonies which may, perhaps, offset the limitations felt in the minds of many who can think of music only as varying arrangements of groups of superimposed thirds. We may freely think of any harmonies as being composed of superimposed intervals of any sort, instead of being shackled by considering every harmony made up of superimposed thirds, or inversions of these. Our vocabulary of harmonies, instead of embracing only chords, will embrace all harmonies.

It may be well here to forestall a possible objection that the chord system is the nearest to the ideal from the physical point of view. It is true that chords are physically the "cleanest" harmonies, i. e., their tones have the simplest vibration ratios. It may be said from this that the system of building up thirds is founded not upon an arbitrary choice, but upon an acoustic basis. But I answer that from this point of view it is immaterial whether we think of major and minor thirds or of 4s and 3s in building up the most important harmonies. If thirds and sums of thirds are the most important intervals, then, after we have learned the chromatic nomenclature, 3s, 4s, 6s and 7s will come to be recognized as the most important intervals. There is no ground for any charge that the chromatic nomenclature is more empiric and less scientific than the chord system.

Again, is not the chromatic nomenclature a simple and an accurate method for naming and classifying any harmony? Instead of grappling with such harmonies as these:

considering them in relation to surrounding harmonies, and in themselves by devious ways, we simply name them chromatically

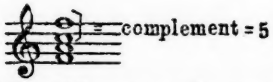
as consisting of superimposed intervals. They are respectively: 6-4-6-4 and 5-5-4-4 harmonies.

Our next task is to study more closely the nature of harmonies and to discover suitable means for systematically finding all of these harmonies.

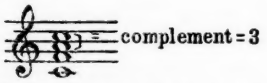
First we will recognize a tone essentially the same whether its vibration frequency is increased or diminished by a power of two; for the ordinary ears hear it as essentially the same. For this reason, we are compelled to regard an inversion of a harmony only as a different position of the harmony and not as a different harmony. In the chord system we decide upon one position of a harmony which we call close fundamenal, namely that position in which the harmony's root is in the base. (There is, however, no distinction made between "open" and "close" fundamental positions.) Likewise, and for purpose of classification, we should decide upon a fundamental position of a harmony in our chromatic system of nomenclature. The choice of a fundamental position, though arbitrary, will be made later in the discussion.

The relations between the tones comprising a harmony are intervals. A harmony may be thought of as a combination of intervals. However, a combination of intervals may form more than one harmony. The *major triad* is a 4-3 harmony, that is it is made up of the intervals 4 and 3; yet if we merely reverse the order of these intervals we have another harmony, the *minor triad*. It is clear then that a harmony is one permutation only of a given combination of intervals.

Now it might be thought that one could form all harmonies existing in our duodecimal system by permutating all possible combinations of intervals in all possible ways. However, by doing this we would calculate an immense number of harmonies, very many of which would only be inversions of each other. To avoid the repetitions occasioned by inversions, in classifying all harmonies, we come to the consideration of an extra interval with each harmony. If we invert the major triad or the 4-3 harmony we have first a 3-5 harmony (commonly called the first inversion) and then a 5-4 harmony (called the second inversion). The extra interval to be considered in this case is the interval 5, which is bounded by the highest tone of the triad and the note an octave above the triad's lowest tone, the triad standing in fundamental position—an interval which will be defined as the *complement*. If the triad is in 4-3 position of f, the complement is the interval c-f:



If the triad is in 5-3 position on c, the complement becomes the interval a-c or 3.



The complement will be denoted by the letter R in the following paragraphs. The term will be employed in the discussion of harmonies having any number of tones or intervals.

Now if A represents the lowest interval of a harmony in close position, that is, such a position in which all tones are within the compass of an octave; B the interval above A; C the interval above B, etc., and R the complement, a harmony could be represented thus:

R
.
.
.
. or ABC....R
C
B
A

The first inversion of this harmony would be represented thus:

A
R
.
. or BC..RA
.
C
B

the second inversion thus:

B
A
R
.
. or C...RAB[6]
.
.
C

[6] Each different letter does not necessarily denote a different sized interval

We can think of all the different positions of the same harmony as arranged clockwise in a circle. Taking the intervals in clockwise order we find a different inversion taking each letter as a starting-point. It is clear that the inversions of a harmony are cyclic rotations of the permutation of intervals forming the fundamental position, whichever it may be. Therefore if we desire to form all possible harmonies from a given combination of intervals, we employ only those permutations of the given combination which are not cyclically related; that is, out of each group of permutations which are cyclic rotations of each other we select only one permutation as representative of a single harmony.

The Number of Harmonies with a Given Combination.

Assume we are given a harmony A-B, in which A and B are of different magnitude. Furthermore let us make A and B such intervals that the complement (R) of A-B, is unequal in magnitude to either A or B. Including R in our letter representation, we denote the harmony as A-B-R. The minor (3-4-5) and major (4-3-5) triads are good examples of such a harmony. We have already seen that the inversions of a harmony are cyclic rotations of its arrangement of intervals. Conversely, if two or more harmonies are cyclically related they are only different positions or inversions of one and the same harmony.

Since we are now engaged in finding how many harmonies can be formed from a given combination of intervals, our problem becomes one of finding the number of cyclically unrelated permutations of the given combination.

Let us experiment with our harmony A-B-R. The permutations of the combination are:

ABR	BRA	RAB
ARB	BAR	RBA

The arrangements in the upper row are cyclically related; likewise those in the lower; yet no one of the permutations in the upper row is related cyclically to any one in the lower row. Hence, only two harmonies: A-B-R and A-R-B, can be formed of the combination A, B, and R. We observe that both harmonies are represented in each vertical group; moreover in each vertical group one letter occupies the same position (i. e., first in this case) in both harmonies while the other two letters are permutated.

Let us experiment in like manner with a combination of 4 intervals (R included as one), in which all the intervals are of

different magnitudes. We represent its parts as A, B, C, and R. Its permutations are:

ABCR	BCRA	CRAB	RABC
ABRC	BRCA	CABR	RCAB
ACBR	BRAC	CBRA	RACB
ACRB	BACR	CRBA	RBAC
ARBC	BCAR	CARB	RBCA
ARCB	BARC	CBAR	RCBA

Here, too, the permutations have been so arranged that those in any one horizontal row are cyclically related, while no permutation in one horizontal row is cyclically related to any one in any other horizontal row. Thus any one of the vertical columns contains all possible harmonies: in this case 3! or 6. It will be noticed here as before, that by retaining one letter in a stationary position throughout while permutating the remaining letters, all possible harmonies are formed from the given combination; for, although many of the permutations of the remaining letters are cyclically related, the stationary letter will bear a different relation to the other letters in each case. In each vertical column one letter is held in the same position, allowing the remaining 3 letters to be permutated in 3! different ways.

With a combination of two intervals (R included) we form 1! or 1 harmony; with three intervals (R included) we form 2! or 2, with four intervals we form 3! or 6, etc. Thus with n different intervals we form $(n-1)!$ different harmonies.

But this formula does not apply to combinations in which two or more intervals are alike; and such combinations are by far more numerous than the others. In fact, no harmony can have as many as five or more different intervals, provided of course that all its tones are bounded by the octave. To illustrate, we find the sum of the five smallest intervals (1, 2, 3, 4, 5) to be 15, which exceeds the octave 12 by 3.

To experiment with harmonies in which two or more intervals are alike let us take the combination: A, B, B, and R. Its permutations are:

ABBR	BBRA	BRAB	RABB
ABRB	BRBA	BABR	RBAB
ARBB	BARB	BBAR	RBBA

Here again the horizontal rows contain cyclically related permutations while the vertical columns do not. We find all three of the

possible harmonies represented in any one vertical group (in each of which one letter has a stationary position throughout). However, since the combination contains two B's there are two "B rows," each of which contains the three harmonies. Thus if one B is held stationary while the remaining intervals are permutated, we obtain six instead of three harmonies; showing that our rule of stationaries[7] holds only for singly occurring intervals. But if we hold A stationary, we permutate B-B-R in $3!/2!$ different ways forming 3 different harmonies.

With the combination A, B, B, R and R we can form the following harmonies: ABBRR, ABRBR, ABRRB, ARBRB, ARBBR, ARRBB, by holding A in the same position and permutating the remaining letters in $4!/2!2!$ (=6) different ways.

The general formula then for the number of harmonies to a given combination in which at least one interval occurs singly is:

$$H\ (\text{harmonies}) = (n-1)!/n_1!\,n_2!\,n_3!\ldots\ldots$$

where n is the total number of intervals (R included) in the combination, and n_1, n_2, n_3, etc., are the respective number of times which certain intervals are found. This is the general formula for the number of harmonies to a combination, as the large majority of combinations contain singly occurring intervals. To find the number of harmonies in a combination containing no singly occurring intervals actual trial must be resorted to; any formula for this would be beyond the scope of this work. As an example of the error to which the formula leads if it is applied to combinations having no singly occurring interval, we will apply it to the combination 3 A's and 3 B's. Here

$$(n-1)!/n_1!\,n_2!\ldots = 5!/3!3! = 1.2.3.4.5/(1.2.3)(1.2.3) = 10/3$$

The result is an impossibility, since a fractional number of harmonies cannot exist.[8]

The Number of Possible Combinations.

We will henceforth regard a harmony as in a *prime* position if its tones are reduced to within the compass of an octave.

[7] That is, the method of holding one interval stationary and permutating the remaining intervals in all possible ways to obtain the several different harmonies.

[8] We remember that throughout the last pages we have considered the complement R as one of the n intervals of a combination. It might be supposed that we could now eliminate R from consideration and thereby make our formula, $n!/n_1!\,n_2!\ldots$. This could not be done generally, since R might be an interval having a magnitude equal to that of another interval (i.e., A, B, or C, etc.) in the combination; in that case it would necessarily figure in the denominator of the fraction.

No harmonies in prime position can consist of less than two or more than twelve tones, in our duodecimal system. Hence no prime harmony can have less than two (R included) or more than twelve intervals. Now the number of combinations possible within the octave could be computed, but the result would be of no value, since in finding the number of harmonies we must treat each combination separately. Furthermore, mere numbers interest us only speculatively while a concrete method of obtaining all harmonies is of real value.

To simplify the task of finding the combinations possible with the twelve units within the octave, we will treat separately those groups of combinations having a different number of tones or intervals. A separate table will be made for each group of combinations having a certain fixed number of intervals. It is, of course, evident that the sum of the intervals of each combination equals the octave 12, since R is always one of the combination's intervals. In the tables the vertical columns contain the respective intervals, varying in magnitude as the number of intervals of the combination permit. In the horizontal rows are found the combinations. If a number greater than 1 is found in a combination, it indicates how many times the interval in whose vertical column it lies is repeated in the combination. Thus a number 3 lying in a column headed by the number 2, indicates that the combination contains the interval 2 (or the whole tone) thrice. The tables follow.[9]

TABLE I; N (NUMBER OF INTERVALS INCLUDING R)=2

	VARIOUS INTERVALS											CALCULATIONS OF HARMONIES	(No. OF HARM.) H
	1	2	3	4	5	6	7	8	9	10	11		
1	1										1	H=(n−1)!=(2−1)!	1
2		1								1		"	1
3			1						1			"	1
4				1				1				"	1
5					1		1					"	1
6						2						"	1
												Total	6

[9] The combination numbers found to the left of the respective combinations are only for future reference.

TABLE III; N=4

	1	2	3	4	5	6	7	8	9	CALCULATIONS OF HARMONIES	H
1	3								1	H=3!/3!	1
2	2	1						1		H=3!/2!	3
3	2		1				1			"	3
4	2			1		1				"	3
5	2				2					By trial	2
6	1	2					1			3!/2!	3
7	1	1	1			1				3!	6
8	1	1		1	1					"	6
9	1		2		1					3!/2!	3
10	1		1	2						"	3
11		3				1				3!/3!	1
12		2	1		1					3!/2!	3
13		2		2						By trial	2
14		1	2	1						3!/2!	3
15			4							By trial	1
										Total	43

TABLE II; N=3

	1	2	3	4	5	6	7	8	9	10	CALCULATIONS OF HARMONIES	H
1	2									1	H=(3—1)!/2!	1
2	1	1							1		(3—1)!	2
3	1		1					1			"	2
4	1			1			1				"	2
5	1				1	1					"	2
6		2						1			2!/2!	1
7		1	1				1				2!	2
8		1		1		1					"	2
9		1			2						2!/2!	1
10			2			1					"	1
11			1	1	1						2!	2
12				3							By trial	1
											Total	19

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TABLE IV; N=5

	1	2	3	4	5	6	7	8	CALCULATIONS OF HARMONIES	H
1	4							1	H=4!/4!	1
2	3	1					1		4!/3!	4
3	3		1			1			"	4
4	3			1	1				"	4
5	2	2				1			4!/2! 2!	6
6	2	1	1		1				4!/2!	12
7	2	1		2					4!/2! 2!	6
8	2		2	1					"	6
9	1	3			1				4!/3!	4
10	1	2	1	1					4!/2!	12
11	1	1	3						4!/3!	4
12		4		1					4!/4!	1
13		3	2						By trial	2
									Total	60

TABLE V; N=6

	1	2	3	4	5	6	7	CALCULATIONS OF HARMONIES	H
1	5						1	H=5!/5!	1
2	4	1				1		5!/4!	5
3	4		1		1			"	5
4	4			2				By trial	3
5	3	2			1			5!/2! 3!	10
6	3	1	1	1				5!/3!	20
7	3		3					By trial	4
8	2	3		1				5!/2! 3!	10
9	2	2	2					By trial	16
10	1	4	1					5!/4!	5
11		6						By trial	1
								Total	80

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TABLE VI; N=7

	1	2	3	4	5	6	CALCULATIONS OF HARMONIES	H
1	6					1	H=6!/6!	1
2	5	1			1		6!/5!	6
3	5		1	1			"	6
4	4	2		1			6!/2! 4!	15
5	4	1	2				"	15
6	3	3	1				6!/3! 3!	20
7	2	5					By trial	3
							Total	66

TABLE VII; N=8

	1	2	3	4	5	CALCULATIONS OF HARMONIES	H
1	7				1	H=7!/7!	1
2	6	1		1		7!/6!	7
3	6		2			By trial	4
4	5	2	1			7!/2! 5!	21
5	4	4				By trial	10
						Total	43

TABLE VIII; N=9

	1	2	3	4	CALCULATIONS OF HARMONIES	H
1	8			1	H=8!/8!	1
2	7	1	1		8!/7!	8
3	6	3			By trial	10
					Total	19

TABLE IX; N=10

	1	2	3	CALCULATIONS OF HARMONIES	H
1	9		1	H=9!/9!	1
2	8	2		By trial	5
				Total	6

TABLE X; N=11

	1	2	CALCULATIONS OF HARMONIES	H
1	10	1	H=10!/10!	1

TABLE XI; N=12

	1	CALCULATIONS OF HARMONIES	H
1	12	By trial	1

Let us tabulate the number of harmonies found in each respective table:

Table	No.	1	Intervals	2	Total No.	H	6
"	"	2	"	3	" "	"	19
"	"	3	"	4	" "	"	43
"	"	4	"	5	" "	"	66
"	"	5	"	6	" "	"	80
"	"	6	"	7	" "	"	66
"	"	7	"	8	" "	"	43
"	"	8	"	9	" "	"	19
"	"	9	"	10	" "	"	6
"	"	10	"	11	" "	"	1
"	"	11	"	12	" "	"	1
					Total		350

We notice that as the number of intervals increases to 6 the number of harmonies increases, while as the number of intervals increases beyond 6 the number of harmonies decreases. Thus, six-tone harmonies (or harmonies of six intervals including R) are most numerous; five- and seven-tone harmonies next in number; four- and eight-tone harmonies next; etc. More harmonies can be formed from combination 4 Table VII than from any other combination. From it we obtain 21 harmonies. Finally we see that the total number of harmonies in the duodecimal system is 350.

The harmonies of least dissonance will be those having the fewest small intervals. There are 9 harmonies having intervals no smaller than 3 (minor third); there are 28 having intervals no smaller than 2 (major second); and there are 55 harmonies having only one interval, 1 (minor second).

So far we have only shown the number of harmonies with each separate combination. Now it remains to show every harmony on the staff. Means have been shown for finding the number of harmonies from each combination. We merely retain a singly occurring interval in one position (preferably the lowest) and permutate the remaining intervals. But in notating harmonies we

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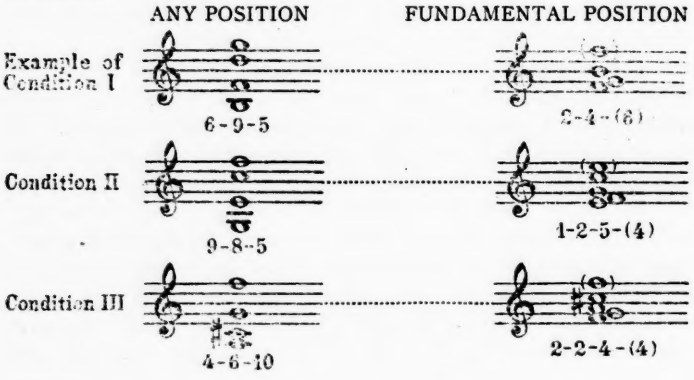
should at least represent them in some standard form; and thus we arrive at the long-delayed decision about fundamental position.

Fundamental Position.

Among the 350 complex harmonies which we have found, there are many—nay, a large majority—which could not be represented as plain chords. Furthermore, since our vocabulary of intervals and harmonies has become a chromatic one, we will no longer attempt to reconcile the limited number of harmonies known as chords, with all of 350 harmonies; hence no attempt to make a chordic position the fundamental form. Arbitrarily we might choose as our fundamental a form in which the *span*, or the interval between the extreme tones of a harmony, is smallest. Or, since we have found it convenient to place any singly occurring interval in the lowest position in forming harmonies from combinations, we might call such a position fundamental. The question is difficult, and although my solution is only arbitrary I believe the fundamental position should be one which satisfies the following conditions:

I. The harmony is prime.
II. The smallest interval (it may be R) occupies the lowest position, and thereby becomes A.
III. In case there exist two or more smallest intervals, the one or more other smallest intervals are nearest A.
IV. In case the two or more smallest intervals are spaced regularly apart, the next smallest interval is nearest A.

Illustrations follow in order respective to the conditions of the definition.

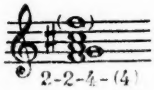


Condition IV

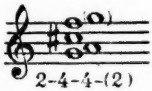
The first example illustrates how a harmony in more or less spread-out position (left-hand column) is reduced to prime position.

The second illustrates how another harmony (9-8-5) is reduced to prime position, following which it is placed so that the smallest interval (1) occupies the lowest position.

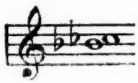
The third illustrates a harmony which, in any prime position, contains two smallest intervals. We are not satisfied with making either of these A; the equivalent of A must be nearest A. Thus in this position:



A's equivalent is nearer A than in the position:



The fourth illustrates a harmony containing two smallest intervals (=1) which are equally separated in any prime position of the harmony. Thus the two intervals (1) are always separated by the interval 5. But because the next smallest interval

lies nearer than 

we consider the interval e-f as A.

Thus in classifying any harmony, only three short steps are necessary. First we reduce its tones to within the compass of the octave; second, we select from the prime positions the fundamental position; third, we name the harmony according to the chromatic nomenclature.

Having determined the fundamental position, we are prepared to write out on the staff all existing harmonies with the help of the previous tables. Every harmony will appear in fundamental form; while each will be respectively named. The harmonies follow:

ALL EXISTING HARMONIES OF THE DUODECIMAL SYSTEM.

LISTED IN FUNDAMENTAL POSITIONS AND BY TABLES.

(The names of the harmonies are written respectively below; the number of combination in which each is found appears above the staff.)

Table I

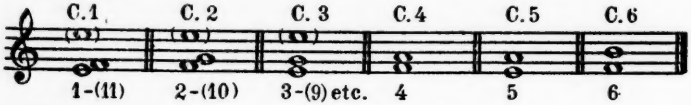
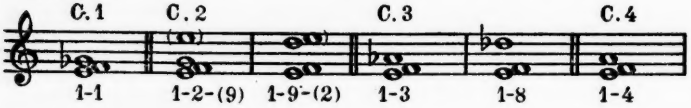
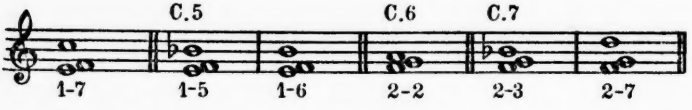


Table II





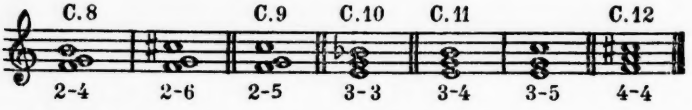
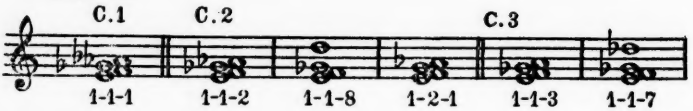
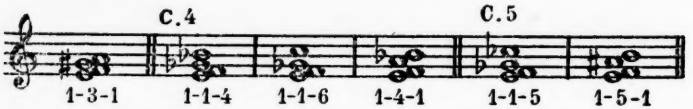
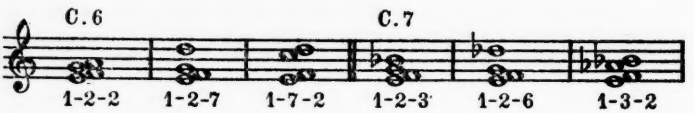
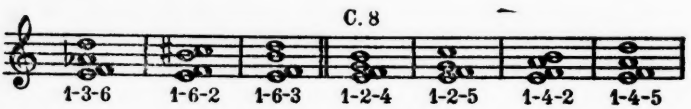
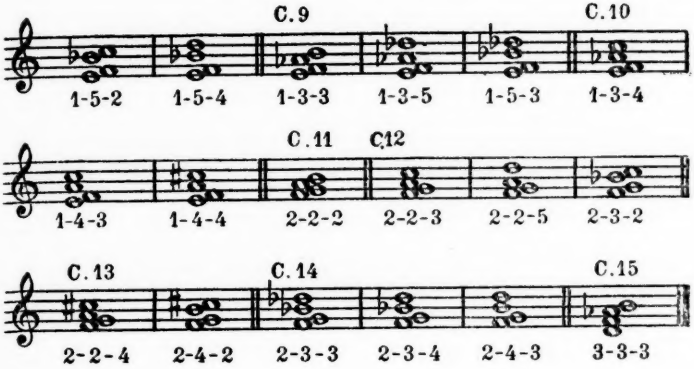
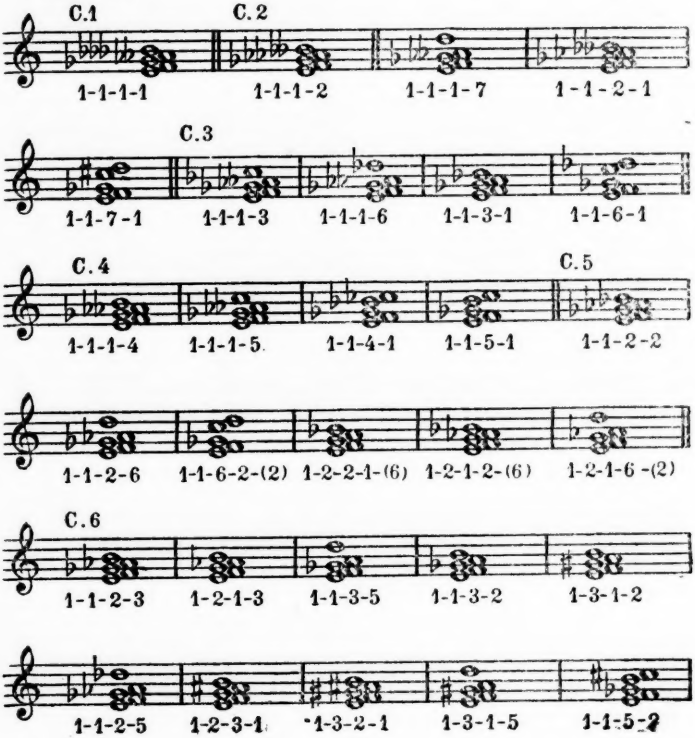


Table III







C.9
C.10
1-5-2
1-5-4
1-3-3
1-3-5
1-5-3
1-3-4
C.11
C.12
1-4-3
1-4-4
2-2-2
2-2-3
2-2-5
2-3-2
C.13
C.14
C.15
2-2-4
2-4-2
2-3-3
2-3-4
2-4-3
3-3-3

Table IV

C.1
C.2
1-1-1-1
1-1-1-2
1-1-1-7
1-1-2-1
C.3
1-1-7-1
1-1-1-3
1-1-1-6
1-1-3-1
1-1-6-1
C.4
C.5
1-1-1-4
1-1-1-5
1-1-4-1
1-1-5-1
1-1-2-2
1-1-2-6
1-1-6-2-(2)
1-2-2-1-(6)
1-2-1-2-(6)
1-2-1-6-(2)
C.6
1-1-2-3
1-2-1-3
1-1-3-5
1-1-3-2
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1-3-2-1
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1-1-5-2

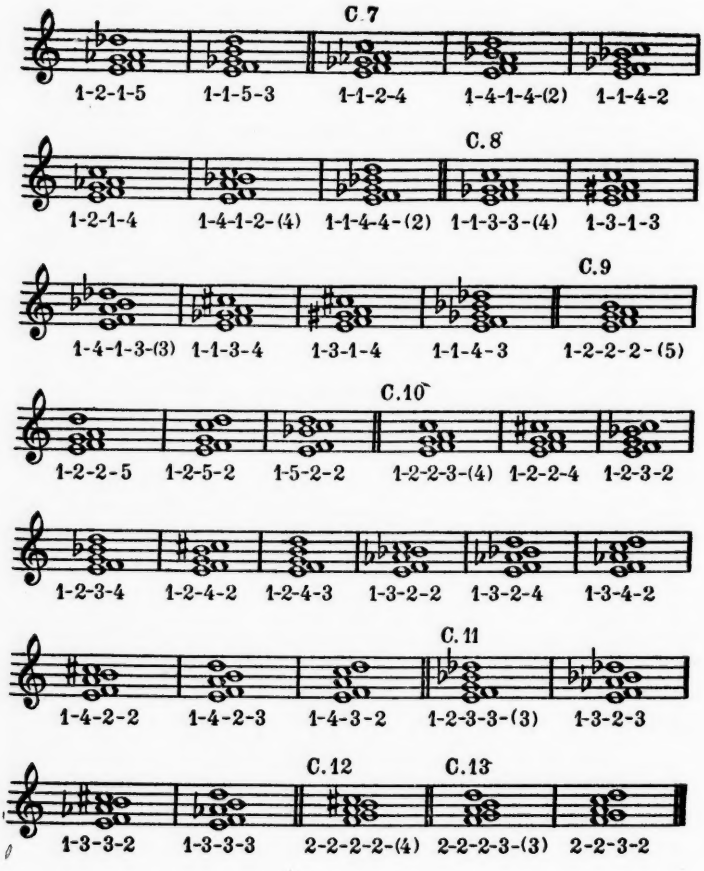
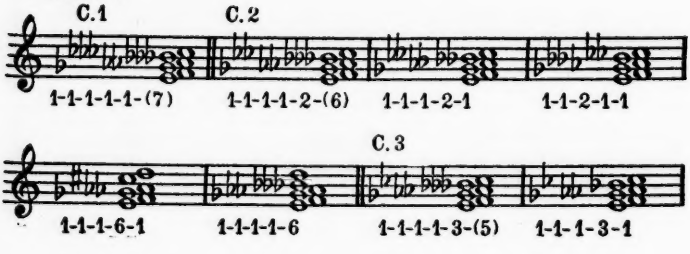
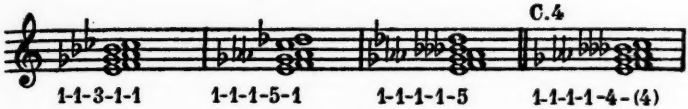

C.7
1-2-1-5
1-1-5-3
1-1-2-4
1-4-1-4-(2)
1-1-4-2
C.8
1-2-1-4
1-4-1-2-(4)
1-1-4-4-(2)
1-1-3-3-(4)
1-3-1-3
C.9
1-4-1-3-(3)
1-1-3-4
1-3-1-4
1-1-4-3
1-2-2-2-(5)
C.10
1-2-2-5
1-2-5-2
1-5-2-2
1-2-2-3-(4)
1-2-2-4
1-2-3-2
1-2-3-4
1-2-4-2
1-2-4-3
1-3-2-2
1-3-2-4
1-3-4-2
C.11
1-4-2-2
1-4-2-3
1-4-3-2
1-2-3-3-(3)
1-3-2-3
C.12
C.13
1-3-3-2
1-3-3-3
2-2-2-2-(4)
2-2-2-3-(3)
2-2-3-2

Table V


C.1
C.2
1-1-1-1-1-(7)
1-1-1-1-2-(6)
1-1-1-2-1
1-1-2-1-1
C.3
1-1-1-6-1
1-1-1-1-6
1-1-1-1-3-(5)
1-1-1-3-1

C. 4

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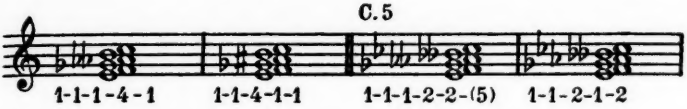
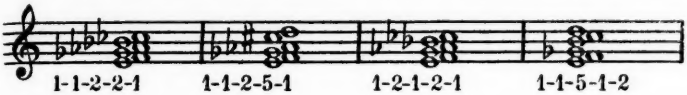
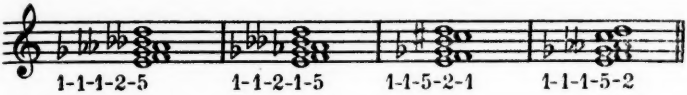
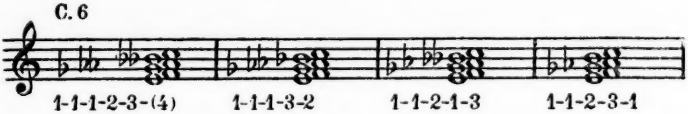
1-1-1-5-1
1-1-1-1-5
1-1-1-1-4-(4)

C. 5

1-1-1-4-1
1-1-4-1-1
1-1-1-2-2-(5)
1-1-2-1-2

1-1-2-2-1
1-1-2-5-1
1-2-1-2-1
1-1-5-1-2


1-1-1-2-5
1-1-2-1-5
1-1-5-2-1
1-1-1-5-2

C. 6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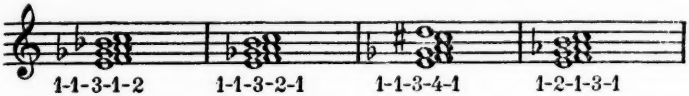
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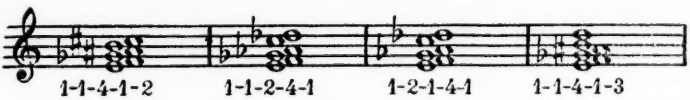
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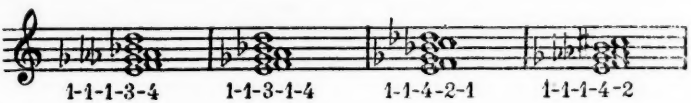
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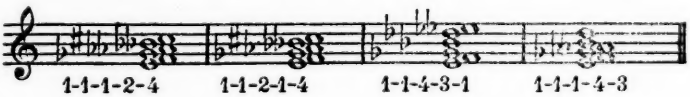
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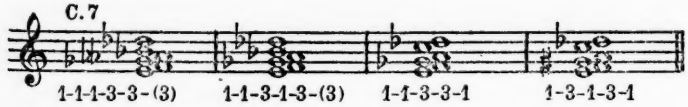

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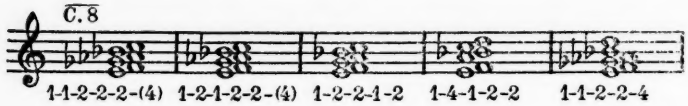

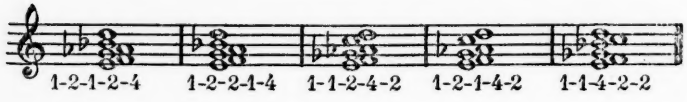

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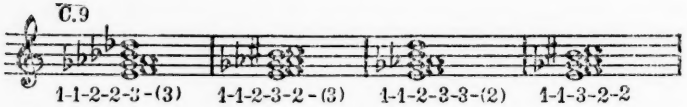

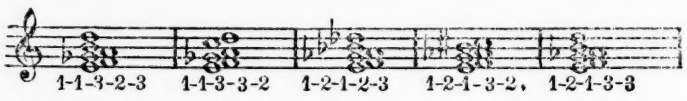

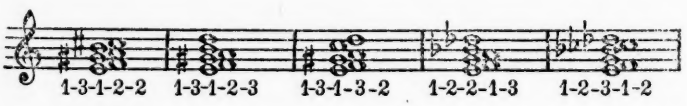

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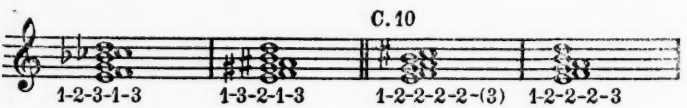

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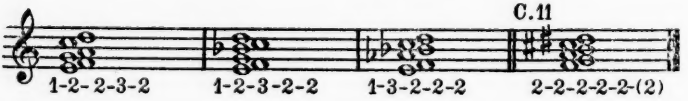

Table VI

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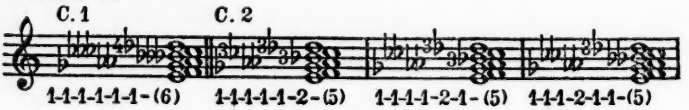
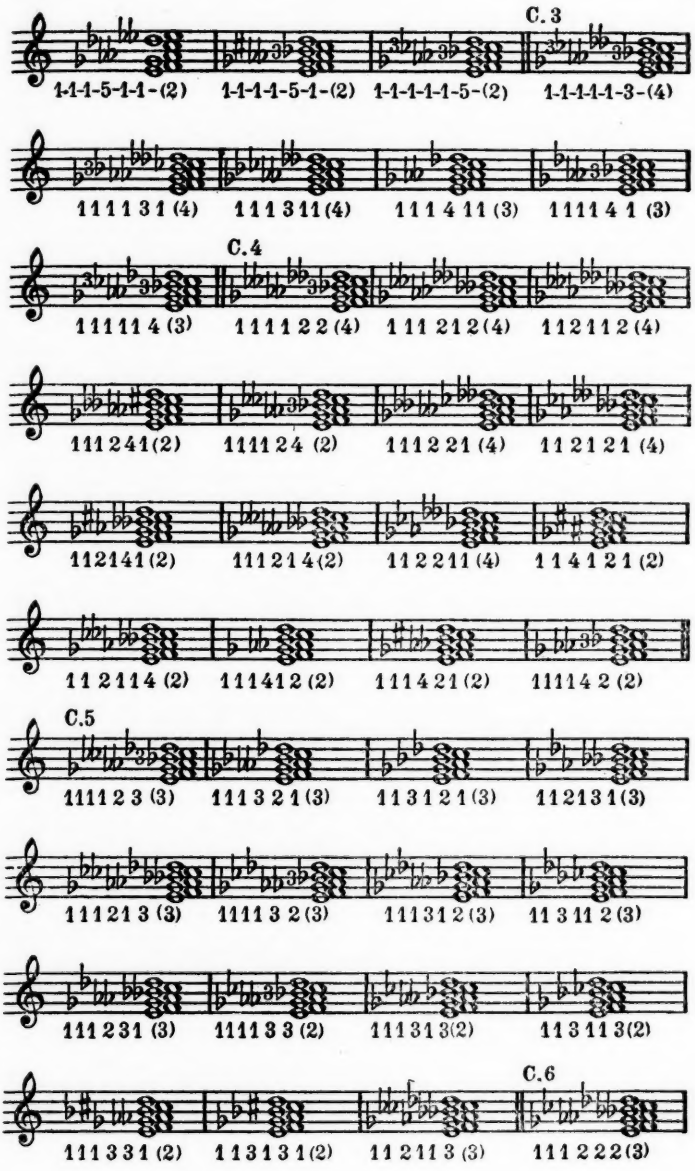
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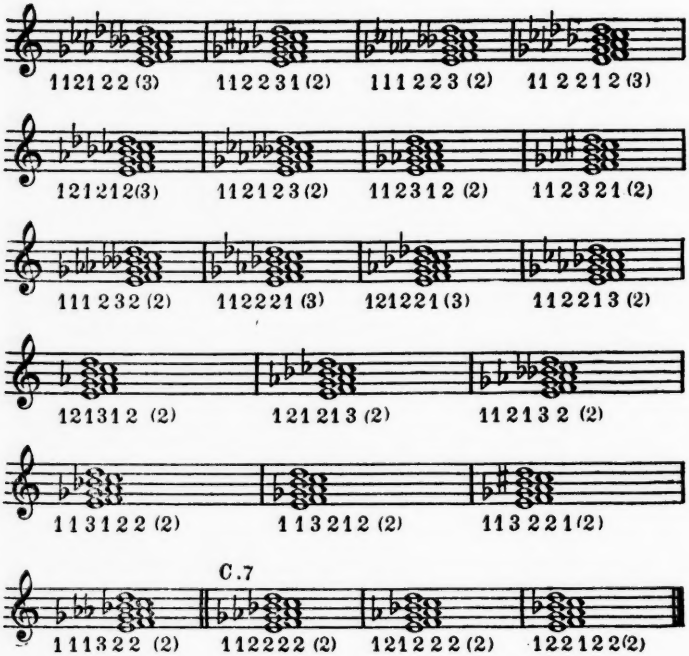
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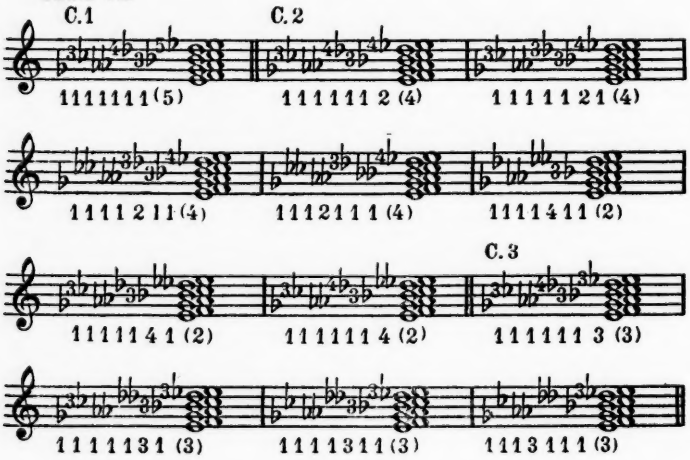
Table VII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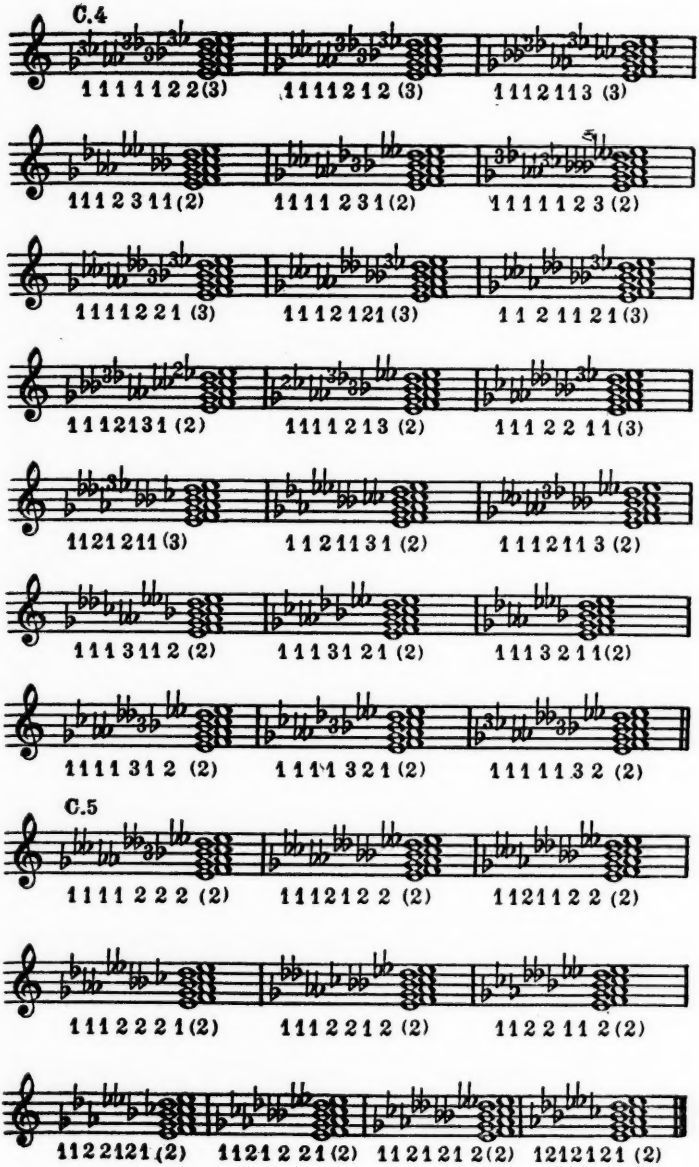
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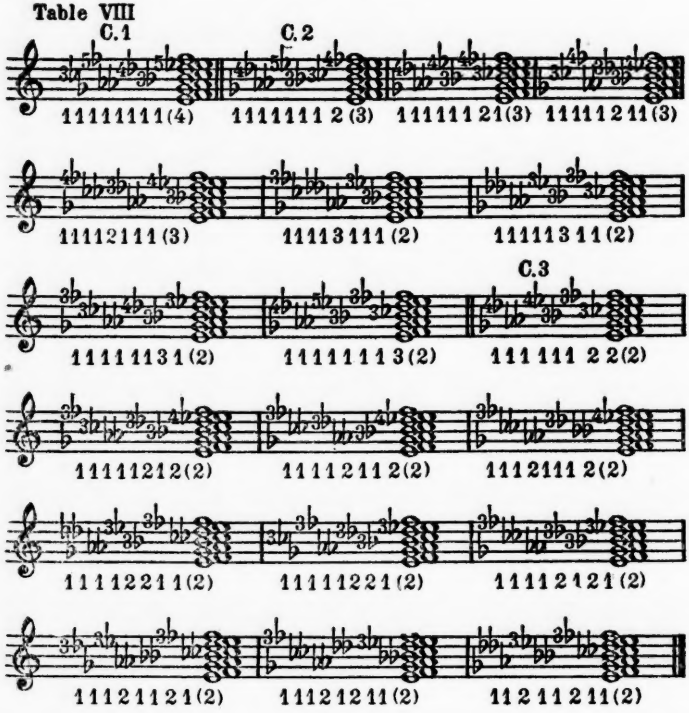

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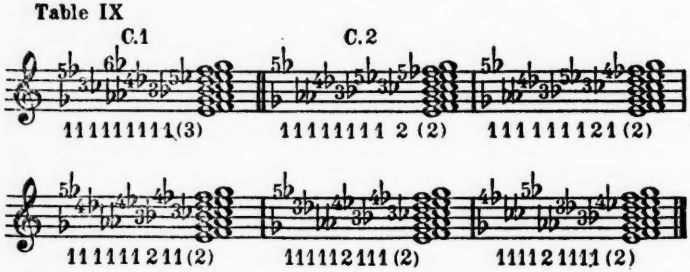

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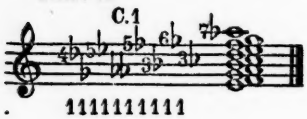

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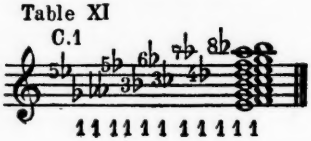


NOTES ON THE NOTATION OF THE ABOVE HARMONIES.

1. R is represented in parentheses where it is included in the notation.
2. The degree most convenient for representation is chosen for the base note.
3. The Number Names under each respective harmony have their integers separated at first by dashes. Later these dashes are omitted.
4. When the number of sharps and flats becomes excessively great, it is written $n\flat$ or $n\sharp$. Thus $\sharp\sharp\sharp\sharp$ becomes $4\sharp$.
5. There are other means of notating some of these denser harmonies. For example, it would be possible to employ two staffs, or double stems. Our present system of notation allows of no better methods.

Inversions of the Harmonies.

Many of these harmonies, especially those of many tones, may sound unesthetic in their fundamental form because of their dissonance, even to an ear trained to an appreciation of the most "ultra-modern" music. A conglomeration of slow beats caused by adjacent tones will, indeed, almost approach a common noise. However, such dissonance can be largely reduced in the same harmony when the tones of the fundamental position are scattered by octaves. Thus many harmonies, seemingly obscure in their fundamental position, become more appreciable to us by inverting them or spreading them out. The different forms and inversions of almost every harmony (made possible by the range of modern instruments) allow of the greatest variety of effects. The number of inversions and positions of most harmonies is astounding. Now, in making our rather superficial study of inversions, we will be obliged to use a few technical expressions; which are enumerated below.

A prime position of a harmony has been previously defined.

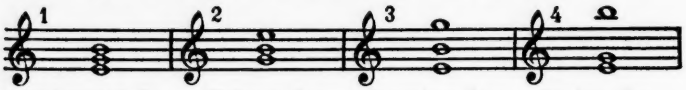
Any prime inversion of a fundamental[10] harmony will be known as *primary* inversion.

An inversion not prime, but containing no interval as great as the octave will be considered a *secondary* inversion.

An inversion containing one or more intervals exceeding the octave in magnitude will be known as a *tertiary* inversion.

An example of each type is given, respectively, below; the three inversions are in the same harmony. Although a form like (3) would, according to the chord system, be considered a fundamental position since the root (e) occupies the lowest position, we will

[10] That is, a harmony in fundaemntal position, according to definition.

find it more convenient to regard any position which is not prime, even though it have the root in lowest position, as an inversion.

Fundamental Pos. Primary Inv. Secondary Inv. Tertiary Inv.

In the following paragraphs, a few general principles are discussed in the form of propositions.

Proposition I. A harmony of n tones has $(n-1)$ inversions of the first degree.

Since a prime inversion can be formed with each tone as a lowest tone, and since $(n-1)$ tones are available as lowest tones (one tone being employed as the lowest tone for the fundamental) it follows that there are $(n-1)$ inversions possible.

In other words, an n-tone harmony has n prime positions.

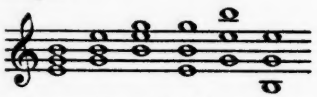
Proposition II. The number of secondary inversions of a harmany of n tones is $(n!-n)$.

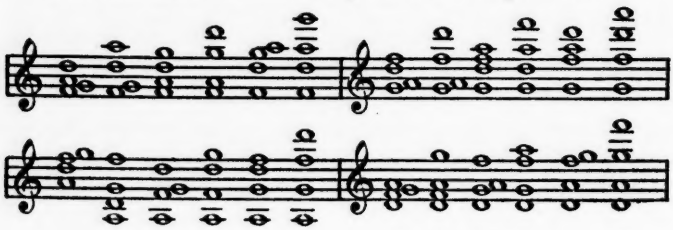
Let us experiment with two-, three- and four-tone harmonies, as follows, allowing no interval between adjacent notes to be as great as 12.

With two-tone harmonies we can form only two or 2! positions which conform to our limitations.

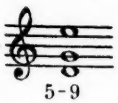
For example:

With three-tone harmonies only 6 or 3! such positions are possible; as for example:



With four-tone harmonies only 24 or 4! are possible:

With 5-tone harmonies 5! or 120 such inversions are possible, etc., etc. With n tones $n!$ positions of this type are possible. But since these positions in which the intervals are less than 12 include n prime positions, the secondary inversions number $(n!-n)$.

It is evident that the number of tertiary inversions is entirely dependent upon the range of the instrument employed to represent them.

By the *span* of a harmony is meant the interval of its extreme tones or the sum of its intervals. Thus the span of

is (9+5) equals 14.[11]

Proposition III. In a harmony of n tones the sum of the spans of its prime positions is $(n-1)$ octaves. Let us take an example from a 4-tone harmony. Adding its prime positions we have

A+ B+ C
B+ C+ R
A + C+ R
A+ B + R

$3A+3B+3C+3R=3(A+B+C+R)=3$ octaves
$=(4-1)$ octaves.

Progressions of Harmonies.

This subject interests us more from a speculative than a practical standpoint, since the possibilities in this direction are well-nigh unlimited, as will be shown. However, if the few suggestions that follow are carried out in limited form practical ends are attainable.

Any harmony may of course be preceded or followed by any other harmonies. Whether the progressions between harmonies sound abrupt or smooth depends partly upon the harmonies in question, partly upon the positions chosen, and partly upon the degree of broad appreciation to which we have been trained. However, smoothness or abruptness of progression does not concern us here, for either may be more desirable according to the character of a composition. The inclination of a composer with ideas certainly

[11] In finding the span of a harmony, R is evidently not included to make the sum 12, since the span of all harmonies would consequently be multiples of 12.

UMI

deserves more consideration than the ever weakening law of theorists.

Let us now calculate the number of progressions of two successive harmonies which can be made with a given number of harmonies, let us say n harmonies. One of these n harmonies placed upon one degree of the chromatic scale can progress to the same harmony placed on 11 other degrees. The same harmony can form 12 progressions with any of the other harmonies given, since any other harmony can be placed on 12 different degrees. And since there are $(n-1)$ such other harmonies, the first harmony can form $(n-1)12$ progressions with the remaining harmonies. Thus the total number of progressions possible between a single harmony and the remaining harmonies is: $11+12\cdot(n-1)$.

But as many progressions are possible with each of the $(n-1)$ remaining harmonies. Hence the total number of progressions of two successive harmonies possible with n harmonies is:

$$n[11+12(n-1)]=(12n-1)n \text{ or } 12n^2-n.$$

Thus, with only 2 harmonies we can form $(24-1)2$, or 46 progressions. With 5 harmonies (which is the limit of vocabulary with many persons, and in which may be included the major triad, minor triad, dominant sept, supertonic sept and leading-tone sept), $(60-1)5$ or 295 progressions of only two successive ones are possible. With the 350 existing harmonies, the possibilities of progression of two at a time are: $(350\times12-1)\,350=1{,}469{,}650$.

The number of possibilities of progressions of three at a time will be $(12n^2-n)12n$, since each progression of two harmonies may be followed by one of n harmonies placed on any one of 12 different degrees of the scale. Thus with 350 harmonies 6,172,530,000 progressions of three are possible.

The general formula expressing the number of progressions possible is:

$$(12n^2-n)(12n)^{s-2}$$

where n is the number of harmonies among which the progressions are to be made, while s is the number of harmonies to be used at a time in a progression.

As mentioned before, the enormous figures just given mean little practically, yet they serve to emphasize the fact that variety is not only desirable but possible; and this is only variety of one kind, harmonic variety.

Melody, rhythm and form are quite as variable as harmony,

and the variability of music is measured as the product of these respective elements of it and is therefore quite beyond the bounds of comprehension. Formerly I shared the foolish and common fear that as more music is written the possibilities of future invention narrow. I actually felt that the field for contemporary composition is narrower than it was a century ago. To-day it seems to me that every great musical work enlarges the field of the future.

"And myriad strains are there since the beginning still waiting for manifestation."[12]

ERNST LECHER BACON.

CHICAGO, ILLINOIS.

[12] Busoni, *A New Esthetic of Music.*

CRITICISMS AND DISCUSSIONS.

THE PRIMITIVE AND THE MODERN CONCEPTIONS OF PERSONAL IMMORTALITY.

In an interesting review of my recent book (*The Beliefs in God and Immortality: an Anthropological, Psychological, and Statistical Study*) in the April number of this journal, it is inadvertently written that the book is "simply a statistical investigation." This statement is true of Part II only. It is not applicable to Part I, for it treats exclusively of "The two conceptions of immortality: their origins, their different characteristics, and the attempted demonstration of the truth of the modern conception." No more does the statement apply to Part III, which discusses "The present utility of the beliefs in personal immortality and in a personal God."

In the first half of the present paper, I set forth very briefly that which I consider the main contribution contained in Part I of my book. In the second half, I give some information concerning the statistics (Part II). J. H. L.

A curious contradiction seems to exist with regard to the origin of the belief in survival after death. It is authoritatively affirmed by anthropologists that that belief is to be found in every tribe now extant. Frazer, less dogmatic, writes that "it might be hard to point to a single tribe of men, however savage, of whom one could say with certainty that the faith is totally wanting among them."[1] And yet historians no less competent in their field than the anthropologists to whom we refer state with disconcerting unanimity that the belief in immortality appeared late among the people from whom Europe drew its civilization. We are told, for instance, that the Israelites' belief in immortality cannot be traced much further back than the beginning of the Christian era. The covenant Yahveh made with his people does not allude to a future life. The nation alone was an object of his care. The great prophets them-

[1] J. G. Frazer, *The Belief in Immortality*, pp. 25, 33.

selves, when they inveigh against sin, care only for the danger therefrom to the existence of the nation. Among the Greeks also the belief in immortality is said to have appeared late. Pythagoras, the Mysteries, and Plato are named as marking the rise of the faith. Of the Romans, Carter says that they did not have the idea of a personal soul: "It was not present at the time of the Punic wars. We see only scanty traces of it in the literature of the Ciceronean age."[2]

These apparently contradictory affirmations may be explained in two ways: either the particular survival-idea expressed in the belief in ghosts, universal among primitive people, had at the beginning of the historical period disappeared from among the nations just mentioned; or the immortality which the historians of these nations have in mind is so different from the primary conception of continuation after death that they disregard that belief.

The first of these two suppositions is not tenable. When the historical period opens, a belief in survival was incontrovertibly present among the peoples of whom the historians we have quoted speak. In the Old Testament traces of polydemonistic belief are definite enough to preclude divergence of opinion. The evidence is just as clear in the case of the Greeks and of the Jews. The Homeric conception of man is of a dual personality composed of a visible earthly being and of its shadow or copy, which manifests its presence in dreams and continues to live in Hades after the severance of death. Jane Harrison has conclusively demonstrated that while the religion of the Olympic gods was in process of formation, and even much later, the Greeks practised rites clearly indicative of the belief in human ghosts.[3]

The idea of *manes*, essential to the religion of the old Romans, is a "vague conception of shades of the dead dwelling below the earth."[4] If one is to believe Lucretius, and there seems to be no reason why he should not be credited in this particular, the Romans were haunted by a dread of the judgment to come.

If the presence at the beginning of the historical period of practices indicative of a belief in survival, in the very people among whom the idea of immortality is said to have appeared late, is no longer a moot point, shall we hold that the kind of continuation

[2] J. B. Carter, *The Religious Life in Ancient Rome*, p. 72.

[3] Jane Harrison, *Prolegomena to the Study of Greek Religion*, 1st ed., p. 11.

[4] W. Ward Fowler, *The Religious Experience of the Roman People*, p. 386.

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after death which our historians have in mind when they deny the existence of the belief in immortality at the beginning of the historical period is so different from the idea entertained by the savage that they do not take that belief into account? The present paper will show that the early conception of survival after death—let it be called the primitive conception—is, as a matter of fact, radically different from the modern conception in point of origin, of nature, and of function.

What was the nature of the primitive belief in the countries bordering the eastern end of the Mediterranean Sea at the period to which it is customary to trace the rise of the belief in immortality? Let us begin with Egypt, the land of the "religion of eternal life." The oldest historical documents we possess, the inscriptions in the passages and chambers of the great pyramids, called the Pyramid texts, belong to an already complex civilization although they date bask to about 3400 B. C. The glimpses of earlier belief found in these texts suffice, however, to indicate the presence of a religion of the underworld according to which the dead continued in unhappy existence under the earth. "The prehistoric Osiris faith," writes Breasted, "involved a forbidding hereafter which was dreaded." In an inscription on a stela addressed by a dead wife to her husband we read: "Oh my comrade, my husband. Cease not to eat and drink, to be drunken, to enjoy the love of women, to hold festivals. Follow thy longing by day and by night. Give care no room in thy heart. For the West Land (a domain of the dead) is a land of sleep and darkness, a dwelling-place wherein those who are there remain."[5]

The Babylonian dead were supposed to dwell in a great cave underneath the earth, the most common name of which was Arula. It "was pictured as a vast place, dark and gloomy.... surrounded by seven walls and strongly guarded, it was a place to which no living person could go and from which no mortal could ever depart after once entering it."[6] For the Babylonians death made all men equal. There were no distinctions of rank in the underworld; kings, priests, conjurers, magicians, and common people, all found themselves together in the dry and dusty *kurnugea*. Everything one touched was dusty. Dust and earth were the food, the muddy water the

[5] A. Wiedemann, *The Realms of the Egyptian Dead*, p. 28.

[6] Morris Jastrow, *Aspects of Religious Belief and Practice in Babylonia and Assyria*, pp. 353, 356, 358.

drink of those living the shadowy life of the underworld.[7] They lived an ineffective, drowsy, starved existence.

Sheol of the Hebrews, like the underworld of the Babylonians, was a place of dread. The shades were forgotten of God. Yahveh was the God of the living, not of the dead. "Go thy way," says Ecclesiastes, "eat thy bread with joy, and drink thy wine with a merry heart....Let thy garments be always white; and let not thy head lack oil. Live joyfully with the wife thou lovest all the days of thy life of vanity....for there is no work, nor device, nor knowledge, nor wisdom, in Sheol whither thou goest."

In Greece also the souls went to the land of the dead bemoaning their lot, for it was wretched. From that dark country souls never returned. Homer draws a repulsive picture of the dead hovering in the dark realm of Acheron, hazily conscious, hollow voiced, weak, and indifferent.

Neither the Egyptians, nor the Babylonians, nor the Hebrews, nor the Greeks could, it seems, think of beings deprived of a vigorous, effective body as enjoying a happy life. The few fortunate individuals who were translated to Elysium or elsewhere without passing through death and lived on happily, had retained their body. The knowledge of the decomposition of the body after death and of the tenuous unsubstantial nature of ghostly apparitions, account naturally enough for the weakness and ineffectiveness attributed to ghosts.

For centuries this repulsive and hopeless belief oppressed the millions from among whom was to rise European civilization. A turning point had, however, been reached at the dawn of the historical period. The primitive belief was apparently doomed, for the leaders in those nations had not only felt the social danger it threatened, and had in consequence begun to deprecate as evil the cult addressed to ghosts, but they had also become clearly conscious of moral cravings, the satisfaction of which death seemed to make impossible.

Regarding the opposition that had arisen to the primitive belief, we may recall that in Israel, the religion of Yahveh was the determined enemy of the cult of the dead in all its forms. And of the Greeks we are told by Jane Harrison that "that which was in the

[7] Friedrich Delitzsch, *Das Land ohne Heimkehr, die Gedanken der Babylonier-Assyrer über Tod und Jenseits*, p. 16. He thinks, however, that as early as the thirtieth century B. C. a distinction in the abode of the shades made its appearance. Some of them lived in peace and comfort in a country provided with water (pp. 18-22).

sixth and even in the fifth century before the Christian era the real religion of the main bulk of the (Hellenic) people, a religion not of cheerful tendance but of fear and deprecation," was the same that Plutarch centuries later, and with him most of his great contemporaries, regarded as superstition. Among the Romans, ghosts had so far lost individuality as to be regarded by modern historians as impersonal forces. The cult had become to an amazing degree a matter of mere conventional behavior.[8] Thus a period of greatly decreased influence among the people of the primitive belief in immortality and of definite antagonism to it by the leaders had arrived.

Simultaneously with this opposition to the old belief, the consciousness of the insufficiency of this life to satisfy the cravings of the heart and the demands of conscience manifested itself in various and increasingly significant ways. One notes precursory signs: for instance, the averred translation of Menelaus to Elysium; of Ganymede to Olympus; of Parnapishtim to an earthly paradise somewhere in Mesopotamia; of Enoch, who was taken up unto his Lord; and of Elijah, who was carried in a chariot of fire by a whirlwind into Heaven. One notes also the appearance among the ancient Hebrews of Messianic hopes; in particular, of the belief in the day of Yahveh when the righteous who had descended to Sheol would arise and participate in the triumph of the nation. The faithful were to be resurrected, not in order to live a blessed, independent existence elsewhere than on this earth, but in order to be *reincorporated in the earthly life of the nation.* These were preliminary manifestations of needs which found their full expression in the modern conception of immortality.

The formation of that conception, as it took place among the Hebrews, is exceedingly interesting. Lack of space forbids anything more than a passing reference to some of the main facts. Job is an early shining instance among the Hebrews of a clear consciousness of the moral incompleteness involved in the limitation of human existence to earthly life. Yet he died without the hope of a blessed immortality. His nearest approach to it is a fleeting persuasion or hope that after death he would enjoy for a moment a vision of God, who would then vindicate his mysterious ways.

The transformation of Yahveh, the God of the nation, into a God maintaining individual converse with the members thereof, and holding each individual, and no longer the nation alone, as morally

[8] W. Ward Fowler, *loc. cit.*, pp. 386-388.

responsible to him, is intimately connected with the establishment among the Jews of the modern belief in immortality. The tragic inner life of Jeremiah shows us how circumstances forced him into individual relationship with Yahveh (chapters xv-xvii). Ezekiel continued the development of Jeremiah's thought. From the existence of an individual relationship with a just God, he drew the unavoidable conclusion that each individual is to be rewarded or punished according to his desert. This new doctrine permeates the Psalms and the book of Proverbs. But when limited to earthly existence, the doctrine is obviously false. Job and the author of Ecclesiastes are up in arms against this truncated truth: "All things come alike to all, there is one event to the righteous and to the wicked; to the good and to the clean, and to the unclean; to him that sacrificeth and to him that sacrificeth not; as is the good, so is the sinner; and he that sweareth, as he that feareth an oath." Ezekiel's doctrine could be made true only by positing another life after death in which the injustice of this life would be repaired. This has remained a chief argument of those believers in immortality who also believe in a benevolent and righteous Creator.

The conception of and the belief in a blessed future existence in which man's deepest and noblest yearnings are to be realized, followed upon the appearance of a deep sense of the worth of these cravings. Whenever, among peoples already familiar with the idea of soul or ghost, these cravings were sufficiently keenly felt, they seemed to have given rise to a belief similar to the Christian belief in immortality.

In Egypt in the religion of the sun-god, long before the book of Job was written, a glorious existence with the god had been conceived. In Greece, Plato taught a lofty doctrine of successive earthly incarnations for the gradual purification of souls from the pollution which comes to them from their association with matter. Ultimately souls entered the glorious world of pure spirits. But this doctrine did not originate with the Greek philosopher. He tells us himself that he got it from the Orphic priests. The Orphic cult was addressed to Dionysos by a sect that had evolved a definite system of religio-philosophic belief, the chief article of which was the double composition of man: one part mortal, coming from the Titans, the other divine. Man's task was to rid himself of the Titanic element, which corresponds to the body, in order to return pure to God. The deliverance of the soul could not be achieved

suddenly nor without the helping mediation of Orpheus, who, let it be noted, demanded a pure life as condition of salvation from rebirth.

The nature of the primary conception of continuation after death gives proof that, unlike the modern conception, it was not born of desires for the fulfilment in another existence of hopes frustrated on this earth. Had it had that origin, it would necessarily have been conceived of in a form designed to satisfy these desires. The nature of the belief and its universality among savages show it to have been imposed, regardless of man's feeling toward it, as irresistibly as the belief in the existence of any object present to the senses.

Differences in origin lead to differences in function. In the primary belief, the ghosts, even those of friends, are on the whole sources of anxiety and fear, and the relations maintained with them aim almost exclusively at warding off their interferences in human affairs. No one loves a ghost and, speaking generally, no one desires to become one. The modern belief is, on the contrary, a vivifying conviction or hope, calling forth the best that is in one's personality.

To consider these two conceptions as bearing to each other the relation of the seed to the fruit, is, therefore, to disregard their respective nature and function as well as their origin. In none of these respects have these conceptions anything essential in common. That is why the primary conception had to be discredited and discarded before the modern one of a glorious life, fulfilling the noblest human demands, could be formed and entertained.

STATISTICS OF CONTEMPORARY RELIGIOUS BELIEFS.

In Part II of my book, I attempted to discover what proportions of the members of a number of influential classes (physical scientists, biological scientists, historians, sociologists, psychologists, and college students of non-technical departments) believe in personal immortality and in the God whose existence is presupposed by all the organized religions, i. e., a God conceived of as acting upon the physical world or at least upon man, at man's request, desire, or desert. It appeared to me of great interest both practically and scientifically to find out definitely the percentages of believers, disbelievers, and doubters among these classes, and to correlate eminence in them and the special kinds of knowledge possessed by their members with these percentages.

I was aware that the statistics of belief so far gathered have little or no statistical value. When, as in the case of the extensive inquiry of the Society for Psychical Research, less than one-third of those who were solicited answered, no particular meaning attaches to the discovery that two-thirds of that one-third believe in immortality. In order to obtain statistics valid for the whole of a group, it is not necessary, it is true, to poll every member of the group. It is sufficient to consider a part of that group, provided that every member of that part or a very high percentage, answer the inquiry, and that the selection of the part investigated be made according to chance. The statistics of that part may then, according to the law of probability, be held valid for the whole group.

The statistical defect from which the inquiry of the Society for Psychical Research suffers, is often combined with an insufficient definition of the belief under investigation. Not long ago some rash person affirmed in the English press that "it is extremely doubtful whether any scientist or philosopher really holds the doctrine of a personal God." Thereupon a Mr. Tabrum collected from among English scientists 140 expressions of opinion on the question, "Is there any real conflict between the facts of science and the fundamentals of Christianity?" But the author did not define what he meant by "fundamentals," neither did he ask his correspondents to state the meaning they attached to that expression. Strange to say, very few thought it necessary to be explicit. Lord Rayleigh wrote, for instance, "I may say that in my opinion true science and true religion neither are nor could be opposed." This has the appearance of a misplaced pleasantry. Any one may make that statement; its significance depends altogether upon what is meant by "true religion." You may have in mind some conception of religion which would tolerate neither the Apostles' nor the Nicean creed, nor even a personal God!

In my own investigation I endeavored to avoid the two major defects illustrated above, and succeeded, I think, in establishing statistics of belief valid for the entire classes named above, so far as the United States is concerned.

The student of human development will be interested in the possibility now opened to ascertain the statistical history of religious beliefs. By instituting at some future time an investigation similar to mine, it would become possible to express with a high degree of exactness the changes that have taken place in the spread of the beliefs here considered.

If I cannot enter here into details as to the statistical method I have followed, the results secured, and their interpretation, I may at least add in conclusion the following figures and some brief information.[9]

Believers in God	Physical Scientists	Biologists	Historians	Sociologists	Psychologists
Lesser Men	49.7	39.1	63.	29.2	32.1
Greater Men	34.8	16.9	32.9	19.4	13.2
Believers in Immortality					
Lesser Men	57.1	45.1	67.6	52.2	26.9
Greater Men	40.	25.4	35.3	27.1	8.8

These figures show that in every class of persons investigated the number of believers in God is less, and in most classes very much less, than the number of non-believers, and that the number of believers in immortality is somewhat larger than in a personal God; that among the more distinguished, unbelief is very much more frequent than among the less distinguished; and finally that not only the degree of ability, but also the kind of knowledge possessed is significantly related to the rejection of these beliefs.

"The correlation shown, without exception in every one of our groups, between eminence and disbelief appears to me of momentous significance. In three of these groups (biologists, historians and psychologists) the number of believers among the men of greater distinction is only half, or less than half the number of believers among the less distinguished men. I do not see any way of avoiding the conclusion that disbelief in a personal God and in personal immortality is directly proportional to abilities making for success in the sciences in question.[10]

"With regard to the kinds of knowledge which favor disbelief, the figures show that the historians and the physical scientists provide the greater; and the psychologists, the sociologists and the biologists the smaller number of believers. The explanation is, I think, that psychologists, sociologists and biologists in very large numbers have come to recognize fixed orderliness in organic and psychic life, and not merely in inorganic existence; while frequently physical scientists have recognized the presence of invariable law in the inorganic world only. The belief in a personal God as defined for the purpose of our investigation is, therefore, less often pos-

[9] These figures are percentages of the number of persons who answered the *questionnaire*.

[10] Concerning these abilities and their influence, see Chapter X.

sible to students of psychic and of organic life than to physical scientists.

"The place occupied by the historians next to the physical scientists would indicate that for the present the reign of law is not so clearly revealed in the events with which history deals as in biology, economics, and psychology. A large number of historians continue to see the hand of God in human affairs. The influence, destructive of Christian beliefs, attributed in this interpretation to more intimate knowledge of organic and psychic life, appears incontrovertibly, as far as psychic life is concerned, in the remarkable fact that whereas in every other group the number of believers in immortality is greater than that in God, among the psychologists the reverse is true; the number of believers in immortality among the greater psychologists sinks to 8.8 percent.

"One may affirm, it seems, that in general the greater the ability of the psychologist, the more difficult it becomes for him to believe in the continuation of individual life after bodily death.

"The students' statistics show that young people enter college possessed of the beliefs still accepted, more or less perfunctorily, in the average home of the land, and that as their mental powers mature and their horizon widens a large precentage of them abandon the cardinal Christian beliefs. It seems probable that on leaving college, from 40 to 45 percent of the students with whom we are concerned deny or doubt the fundamental dogmas of the Christian religion. The marked decrease in belief that takes place during the later adolescent years in those who spend those years in study under the influence of persons of high culture, is a portentous indication of the fate which, according to our statistics, increased knowledge and the possession of certain capacities leading to eminence reserve to the beliefs in a personal God and in personal immortality."[11]

To the statistical data are added a large number of letters from my correspondents and a somewhat full study of the religious ideas of students. These together with the statistics make a picture of the present religious situation both vivid and relatively exact.

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[11] *The Belief in God and Immortality*, pp. 277-281.

NOTES ON RECENT WORK IN THE PHILOSOPHY OF SCIENCE.

Federigo Enriques ("Sur quelques questions soulevées par l'infini mathématique," *Rev. de métaphysique et de morale*, March, 1917, Vol. XXIV, pp. 149-164) points out that experience, when it is idealized by reason, puts before us two kinds of infinity, the actually and the potentially infinite; suppose then "that we are potentially given by thought an infinity of objects, the question arises as to whether there is any reason to consider as logically defined a new object of thought which expresses the totality or the limit of these objects even when they are not constructed with respect to a concept of the kind which we suppose to be given *a priori*." The answer to this question depends on a fundamental tendency of the mind; it will be negative or in some degree positive according as we feel ourselves borne toward nominalism or toward realism. Realist doctrine—at least in mathematics—in its first historical form rested on the assumption that a simple passage to the limit was always possible, and this was gradually destroyed by the progress of the infinitesimal calculus. The realist doctrine in its second historical form rests on the principle that (p. 159) "every infinity of virtually defined objects may be considered as a totality forming a class and constituting a new logical object." As distinguished from realism in its first form, in this new realism we conceive that the properties of the new object are absolutely new and that thus we cannot state them *a priori* by an induction extended from the finite to the infinite. This new form of realism is due, in its mathematical form, to Georg Cantor, but (p. 159) "the philosopher B. Russell has developed in the widest sense the philosophical consequences of the realism thus introduced into mathematics." The various paradoxes of mathematical logic have led to the conclusion that there are, in certain cases, no such things as classes of perfectly definite objects; and this realism, in its second form, is partially unsuccessful. On p. 163 we read that "the principle of an infinite number of choices is adopted by Russell and by Zermelo," and so we are apparently again forced to the conclusion that Enriques is quite unaware of the tendency shown by Russell's work published since 1905. Since the question is rather important, per-

haps the present reviewer may be forgiven for dwelling on some work on the paradoxes in question since 1903.

In Russell's *Principles of Mathematics* (Cambridge, 1903) there was not any definite suggestion that the concept of class should be restricted, though there was certainly a more or less vague feeling that some classes should be excluded (see *Monist*, January, 1912, Vol. XXII, pp. 153, 157-158; and January, 1917, Vol. XXVII, p. 144). The merit of perceiving that a restriction was necessary and of attempting to give a criterion to decide which classes were legitimate seems due to Jourdain, in a paper published in 1904 (see *Monist*, January, 1917, Vol. XXVII, pp. 148-150). The question as to the *being* or *not-being* of a class is totally different from the question of the possibility of an infinite series of acts of selection,—which, by the way, neither was nor is assumed by Russell, though it is believed in by Zermelo and many others. The merit of being the first to publish an explicit recognition of the postulate involved in this assumption is due to Zermelo in 1904, and the questions relating to Zermelo's axiom have been frequently confused with, for example, Jourdain's "proof" by even eminent people; though both Russell and Jourdain pointed out repeatedly that the questions involved are quite different. In 1905 and later Russell published papers gradually showing how it was possible to work through a great deal of Cantor's theory without assuming that there are such things as classes at all, and a thorough exposition of this theory is one of the most important parts of Whitehead and Russell's *Principia Mathematica* (Vol. I, Cambridge, 1910). Thus it is obvious that this theory of Russell's not only makes the considerations of Jourdain and some others quite superfluous, but also makes such criticisms as that of Enriques entirely off the point (cf. *Monist*, January, 1917, Vol. XXVII, pp. 145-148).

This historical sketch is also relevant to our consideration of a recent paper by Dmitry Mirimanoff ("Les antimonies de Russell et de Burali-Forti et le problème fondamental de la théorie des ensembles," in *L'enseignement mathématique*, 1917, Vol. XIX, pp. 37-52). The author remarks (p. 48) that we can find in the works of Bertrand Russell, Henri Poincaré, and Julius König (*Neue Grundlagen der Logik, Arithmetik und Mengenlehre*, Leipsic, 1914) "a profound logical and psychological analysis of the Cantorian antinomies and of the notion of class," but that he "will not have any need of this analysis for the end which" he has in view. His

article is characterized by the following quotation from p. 38: "People believed, and it seemed quite evident, that the existence of individuals necessarily implies that of the class of them, but Burali-Forti and Russell showed by different examples that a class of individuals may be non-existent, although the individuals exist. As we cannot refuse to accept this new fact, we are obliged to conclude from it that the proposition which seemed evident and which was believed always to be true is only true under certain conditions. And then arises the problem which we may regard as the fundamental problem of the theory of aggregates: What are the necessary and sufficient conditions for the existence of a class of individuals?" On this confusion between the ideas of existence and entity, see the article quoted above in *The Monist* for January, 1917. The author gives a solution of this problem for the particular case of classes that he calls "ordinary" classes, and his deductions rest on three postulates (p. 49) which are applied by some in the study of problems of the theory of aggregates. Further, the examples of Russell and Burali-Forti are modified (pp. 39-48) in a way that seems advantageous to the author, and the author announces (pp. 39, 52) his intention to give in a future article the reasons which determined him not to adopt in this paper the theory of König. The criterion which the author arrives at (pp. 48-52) for deciding whether individuals have a class or not is practically that suggested by Jourdain in 1904: individuals have a class if they can be arranged in a segment of the series W of all ordinal numbers and not if they cannot be so arranged. It is not worth while to enter into a criticism of this suggested criterion, which in any case has become quite superfluous through the work of Russell referred to above. Mirimanoff (p. 52) regrets that it has been impossible for him to become acquainted with work that has appeared since the beginning of the war. If he had read—which he nowhere gives any sign of having done—the works referred to above of 1904 to 1910, we do not think that it would have been necessary to write this paper.

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In the *Revue de métaphysique et de morale* for January, 1917, there is an address given by the late Victor Delbos on the general characteristics of French philosophy. A part of the late Louis Couturat's *Manuel de logistique,* which he wrote about 1906 but which is not yet published, is printed. These extracts form the greater part of the second chapter of this book, and are on the

logical relations of concepts and propositions. An interesting fact about them is that the work of Frege seems to have influenced Couturat to a greater extent than was the case with Couturat's *Principes* of 1905. This contribution is fairly elementary, and does not deal with those paradoxes which are, perhaps, of the greatest interest to logicians, although it just mentions them. F. Colonna D'Istria writes on the logic of medicine according to Cabanis's *Rapports du physique et du moral de l'homme*. Arnold Reymond makes a critical study of the new and recast edition of Edouard Claparède's *Psychologie de l'enfant et pédagogie expérimentale* (Geneva, 1916). Thomas Ruyssen writes on "an idea in peril: humanity, humanitarianism, humanism." Finally there are obituary notices of Théodule Ribot (1839-1916), the eminent psychologist, and Henri Dufumier.

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In the number of the *Revue de métaphysique et de morale* for May, 1917, A. Espinas deals with the initial idea of the philosophy of Descartes, and the late Victor Delbos's lecture on method in the history of philosophy forms the second of his three lectures on the history of philosophy. A manuscript by the late Louis Couturat, which was certainly written before 1902 and which will not form part of the projected *Manuel de logistique*, is printed here and is on the algebra of logic and the calculus of probabilities. Finally Alessandro Padoa has a paper on the consequences of a change of primitive ideas in any deductive theory whatever.

* * *

In a paper on the "infinite numbers" which Bernard le Bovier Fontenelle tried to introduce in his *Eléments de la géométrie de l'infini* (Paris, 1727), Branislav Petronievics ("Sur les nombres infinis de Fontenelle," *Rendiconti della R. Accademia dei Lincei* [*Classe di scienze fisiche, matematiche e naturali*], Vol. XXVI, 1917, pp. 309-316) tries to show that this "first attempt at a rational theory of infinite numbers," although it is obviously full of contradictions which were at once pointed out by MacLaurin and others, possesses a historical value when compared with the theories of Cantor and Veronese. Cantor's theory has, says Petronievics, an arithmetical starting-point, while that of Veronese has a geometrical one; and Veronese establishes that there is no point on an infinite straight line which corresponds to the first transfinite ordinal number of Cantor, so that "geometrical application of the transfinite

numbers of Cantor is not possible." In spite of the fact that Fontenelle introduced his "infinite number of all finite numbers" as "the last one" of this series itself, Petronievics maintains that the theory of Fontenelle has a historical value in that it "may be regarded as the common source of the theories of Cantor and Veronese," and that "it is not impossible to suppose, in view of the likenesses between the theories, that Cantor and Veronese both arrived at establishing the principles of their theories when trying to avoid the flagrant contradictions into which Fontenelle fell."

There does not seem to the reviewer to be the smallest ground for supposing that Cantor was led to his theory either by reading Fontenelle or by setting out deliberately to generalize arithmetic. Indeed, one of the points of the long introduction to the translation of Cantor's later papers published under the title of *Contributions to the Founding of the Theory of Transfinite Numbers* (Chicago and London, 1915) is to show that Cantor was compelled to generalize the idea of number as a consequence of the natural development of his process of "derivation" of geometrical point-sets. In this extension it appeared clearly that the transfinite numbers began *beyond* the whole series of finite numbers in opposition to Fontenelle's notion mentioned above. Fontenelle says on page 30 of his book: "We must not be frightened at the words 'last term' in this connection. It is a last *finite* term that the natural series of numbers has not, but not to have a last finite term is the same thing as to have a last infinite term." This is a charming way of turning a universal negative proposition into a particular affirmative one. It seems that the first time that Cantor spoke more or less publicly of Fontenelle's theory was in a letter of 1886 (cf. *Zur Lehre vom Transfiniten*, Halle, 1890, p. 50), and therefore long after he had founded his absolutely different theory. That hearing of one theory may have been the psychological cause of Cantor's thinking about a fundamentally different theory is of course both possibly and probably irrelevant, but there is no ground for supposing that even this happened. It is a mistake to say that ordinal numbers cannot have geometrical applications: an illustration of the way such numbers can appear is given by this: To the series on the x-axis formed by the points 1, 1/2, 1/3, ..., $1/n$,...., the point 0 bears exactly the same relation as the first transfinite ordinal does to the finite ordinals in order of magnitude. For Cantor's remarks on Veronese and Veronese's reply, we may quote the above

Contributions (pp. 117-118). A purely analytical exposition of the infinite and infinitesimal numbers of Veronese was given by T. Levi-Civita ("Infiniti e Infinitesimi attuali," *Atti R. Istituto Veneto*, 1892).

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Florian Cajori ("The Zero and Principle of Local Value used by the Maya of Central America," *Science*, Vol. XLIV, 1916, pp. 714-717) draws attention to the fact, hitherto apparently unnoticed by mathematicians, that the Maya of Central America and southern Mexico seem to have used a symbol for zero and the principle of local value much earlier than any one else. The material for Cajori's remarks is furnished by *An Introduction to the Study of the Maya Hieroglyphs*, by Sylvanus Griswold Morley (Bulletin 57 of the Bureau of American Ethnology, Washington, 1915). The early Babylonians possessed the principle of local value, but so far as we know did not possess a zero. About 200 B. C. they did have a symbol for zero, which, as Smith and Karpinski (*Hindu-Arabic Numerals*, Boston, 1911, p. 51) say, was "not used in calculation, nor does it always occur when units of any order are lacking." They did not employ it systematically in writing numbers and not at all in performing computations. The Hindus certainly did not use their symbol for zero systematically before probably the sixth century A. D., and the earliest undoubted occurrence of zero in Indian numerals is A. D. 876 (cf. also G. R. Kaye, *Indian Mathematics*, Calcutta, and Simla, 1915, p. 31, for the date of the appearance of the principle of local value in India). Now, it seems that the Maya used the zero and the principle of local value at the beginning of the Christian era if not much earlier. "As far as is known, the Maya used their numeral systems only in the counting of time as it arose in their calendar, ritual, and astronomy." Of the several Maya numeral notations the one which is of greatest interest as embodying the principle of local value and the symbol for zero is found in Maya codices but not in their inscriptions. The number system was vigesimal, with the solitary break that 18 (and not 20) *uinals* make 1 *tun*, and the symbols 1 to 19, both inclusive, are expressed by bars and dots. Each bar stands for five units and each dot for one unit, and the dots are written above the bars. Thus 19 is written as three bars above one another and four dots on the top. "The values of the bars and dots are *added* in each case. The zero, which plays a leading part in the notations found on inscrip-

tions as well as those on codices, is represented in the codices by a symbol that looks roughly like a half-closed eye.... In writing 20... the principle of local value enters for the first time. It is expressed by a dot placed over the symbol for zero. The numerals are written, not horizontally, but vertically, the unit of the lowest order or value being assigned the lowest position. Accordingly, 37 was expressed by the symbols for 17 (three bars and two dots) in the *kin* [units] place and one dot, representing 20, placed above the 17, in the *uinal* place. The number 300 is expressed by three bars drawn above the symbol for zero ($3 \times 5 \times 20 = 300$). The largest number which can be written by the use of only two places or positions is $17 \times 20 + 19 = 359$. To write 360, the Maya drew two zeros, one above the other, with one dot higher up, in third place. Using three places to represent *kins, uinals,* and *tuns,* they could write any number not larger than 7199. Proceeding in this way the Maya wrote numbers in very compact form. The highest number found in codices is 12,489,781.... The symbols representing this number occupy six different places, one above the other.... The second numeral notation that was fully developed and employed by the Maya is found in their inscriptions. It employs the zero, but not the principle of local value. Special glyphs are employed to designate the different units. It is as if we were to write 1203 as: '1 thousand, 2 hundreds, 0 tens, 3 ones.'"

The question as to the origin of the arithmetical notation that we call "Hindu-Arabic" has received a new and unexpected contribution from Carra de Vaux ("Sur l'origine des chiffres," *Scientia,* Vol. XXI, 1917, pp. 273-282.) With the Arabs these figures are called *hindi* and the usual meaning of this word is "Indian." Now, the Arabian historian Masoudi, writing in 943 A. D., said that the Hindu numerals were discovered by a congress of wise men gathered together by the powerful and wise king Brahman under whom arts and sciences flourished. "People who are even slightly familiar with the history of philosophy will recognize this at once as a neo-Platonic legend," and a mention of the "Era of the Creation" allowed de Vaux to conclude that this legend is Persian, for that is a Persian era. Also in the work of the other Arabic historian, Albirouni, we have a remark that the numerals came from India, that is vague and contrasts strongly with his usual exactness. This seems to show a lack of definite knowledge on Albirouni's part.

The author then examines the word *hindi,* and comes to the

conclusion that it is a form of *hindasi* whose root is the Persian *ènd* and which means metrical or arithmetical. Thus "signs of *hind*" means "arithmetical signs" and not "signs of India." Consider this example: Apollonius of Perga, who was not an Indian, was said to be *el-hindi* in some Arabic manuscripts, thus this word must evidently be translated as if it were *el-hindasi*, the geometer or engineer. It is to be noticed that in Arabian treatises the abacus is called *takht* which is a Persian name. Thus de Vaux concluded that the numerals originated in the Greek world, and the history of their slow diffusion is easier to explain if we admit that they are a neo-Platonic "or (*soit*) neo-Pythagorean" invention, for the Pythagoreans are well known to have had a taste for secrecy. From Greece the numerals passed to Persia and the Latin world, and from Persia to India and afterward to Arabia.

The figures themselves were not formed from letters of the alphabet, but directly by means of very simple conventions. These characters are due to the neo-Platonists and were known in the schools of Persia before they were in Islam, and it is there that the Arabs found them. From Persia also they passed into India.

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In the first number (January, 1917) of Vol. XXIV of the *American Mathematical Monthly*, the official journal of the Mathematical Association of America, there is an important paper by Edward V. Huntington on "The Logical Skeleton of Elementary Dynamics" (pp. 1-16). The object of the article is to outline the logical structure of elementary dynamics. Any logically developed science must begin with undefined concepts, in terms of which all the other concepts of the science are expressed; and in this case Huntington takes them to be: (1) Space and time, with the derived concepts of velocity and acceleration; (2) Forces, "as suggested by the tension and compression in our own muscles" (p. 1) and "as measured by a spring balance" (p. 3); and (3) Inert material bodies, on which our forces act. The unproved propositions, from which all the other propositions of the science are derived, are only four in number. The first is (p. 4) that "a free particle, when acted on by a force, acquires an acceleration in the direction of the force; furthermore, if a given particle is acted on at different times by two forces F and F', and if a and a' are the corresponding accelerations, then $F/F' = a/a'$; that is, the accelerations are proportional to the forces." This principle "is best regarded as a scientific hypothesis,

the truth of which has been abundantly verified by experiment." It contains the answer to the fundamental question of dynamics: "If a force gets hold of a free particle, and proceeds to act on it, what happens to the particle?" The second and third principles, which cover the case of the particle being acted on by several forces simultaneously, are the principle of the vector addition of forces and the principle of the independence of two perpendicular forces (p. 5). For any given body the ratio of the force to the acceleration produced is constant, and the value of this ratio "is a characteristic of the body, which may be called its inertia" (p. 4). "The *weight* of a body in a given locality with respect to a given frame of reference is best defined as the force required to support the body at rest with respect to that frame in the given locality" (p. 5). We then have the theorem that "if W is the weight of a body in a given locality and g is the falling acceleration of that body in the same locality, then the ratio W/g is independent of the locality, and is a correct expression for the inertia of the body" (p. 6). The proof for the case of fixed axes follows immediately from the first two principles, and a proof for the case of moving axes "belongs later in the course." The theorem (p. 6) that, in any given locality, the falling accelerations of all bodies are equal, "can be proved from general considerations; or, if preferred, it may be accepted as an empirical fact." The words "mass of three pounds" are taken (p. 7) as meaning the same thing as that the body in question "has a weight of 3 lbs." in the standard locality; the weight being a multiple of the unit of force (lb. in the British system). The fourth and last fundamental principle is the principle of action and reaction (p. 8): "When two particles are in contact with each other, or attract or repel each other according to any law like that of gravitation or magnetism, the interaction between them may be represented by a pair of twin forces, equal in magnitude and opposite in direction—one of the twins acting on one particle and one on the other, along their joining line." The definition of the "centroid or center of mass" is as a "weighted average" (p. 8), and the theorem on the motion of the center of mass is then proved. It is emphasized (p. 11) that the difficulties outside the four fundamental principles are of a mathematical sort. It will be seen that the system is based on fundamental units of force, length, and time instead of on units of mass, length, and time, and the author shows by tables (pp. 15, 16) the higher practical value of the system of

derived units advocated by him. This is "one of the best arguments in favor of the use of force rather than mass as the principal undefined concept of dynamics. The only reason why the text-books so insistently base their derived units on mass instead of on force is apparently that a standard lump of metal is easier to preserve in a museum than a standard spring balance. But this is no argument for the logical priority of mass over force. As a matter of fact, the fundamental unit of force is as easy to preserve as the fundamental unit of mass, though the method of doing so does not consist in simply storing away a spring balance" (p. 16). The name of the unit of force, in the British system, is "pound" (lb.) and is defined as "the force required to support a carefully preserved lump of metal, called the 'standard pound avoirdupois,' in vacuo, in the standard locality" (p. 14).

The reviewer would remark that, though mass under the name "inertia" is a derived unit in the system advocated by Huntington, we have to use the unit of mass as a practical means of preserving the unit of force. It is quite true that this fact is no argument for the logical priority of mass: it is merely a question of practical convenience. But in either of the two systems there seem to be, at first sight, three fundamental undefined units, and so, from a logical point of view, nothing is gained by replacing mass by force as a fundamental unit. But let us look at the matter more closely. As we have learned from the work of Mach (see, e. g., his *Mechanics*, 3d edition, Chicago and London, 1907, p. 243), "mass-ratio" can be defined in terms of the mutual accelerations of bodies, and so there seems to be a logical advantage in the system in which force is not regarded as fundamental, but is defined. Further, even in Huntington's system, "force" can be defined by the property that F/a is constant, and then his system and Mach's seem to be identical. By the way, the forces we use in dynamics are not all "suggested by the tension and compression in our own muscles": the attraction of the sun is not; and it is both logically objectionable and rather confusing to a student to have various concepts with a single name.

It must be added that stress is (pp. 7-8) rightly laid on the difficulty which beginners have in realizing that, when a particle describes a curve, there is actually an acceleration along the normal.

There is an interesting review of Florian Cajori's *William Oughtred* on pp. 29-30 written by Louis C. Karpinski, where it is stated that Cajori's remark that Napier was the first to use a decimal

point (1616 and 1617) is an error: it was first used by Pitiscus in 1612.

In the February number is a short account (pp. 54-55) of Cajori's presidential address to the annual meeting of the Mathematical Association of America in New York City at the end of 1916 entitled "Discussions of Fluxions from Berkeley to Woodhouse." This address was a shortened account of a book by Cajori which will appear before very long in the "Open Court Classics of Science and Philosophy." At a meeting of the Council it was decided, among other things, to consider the question of possible assistance for the *Revue semestrielle* and the *Jahrbuch über die Fortschritte der Mathematik* in view of the difficulties that must attend publication owing to the war (p. 64). Very much the same discussion was held by the Chicago Section of the American Mathematical Society (p. 97). David Eugene Smith, in "On the Origin of Certain Typical Problems" (pp. 65-71), has a very learned article on the history of the problems of (1) filling a cistern of water, (2) the *Josephspiel,* or the problem of the Turks and Christians, (3) the testament of a man about to die, dividing his estate, (4) the problem of pursuit.

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Frank Egleston Robbins (*Amer. Math. Monthly,* March, 1917, Vol. XXIV, pp. 121-123) gives an interesting and critical review of George Johnson's partial translation of and commentary on the *Introduction to Arithmetic* of Nicomachus, in his dissertation on *The Arithmetical Philosophy of Nicomachus of Gerasa* (Lancaster, Pa., 1916). The essay of Nicomachus is of course the earliest extant attempt at a systematization of the Greek science of theoretical, as distinguished from practical, arithmetic.

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In the *American Mathematical Monthly* for May, 1917, W. H. Bussey ("The Origin of Mathematical Induction," Vol. XXIV, pp. 199-207) points out that Moritz Cantor is mistaken about the use of complete induction both in his *Geschichte der Mathematik* (Vol. II, 2d ed., 1900, p. 749) and his note, correcting this mistake, on Maurolycus in the *Zeitschrift für mathematischen und naturwissenschaftlichen Unterricht* (Vol. XXXIII, 1902, p. 536). In the note Cantor said that he had found that Maurolycus described and used the method in his *Arithmeticorum libri duo* (Venice, 1575), and that Pascal had expressly borrowed the method from Mauroly-

cus; Bussey shows that there is an error, in a minor respect, in Cantor's references. Bussey quotes a number of Maurolycus's theorems. In his sixth proposition, that any integer (n) added to the preceding one is equal to the "collateral odd number" (the nth odd number, $2n-1$), Maurolycus's proof, freely translated is: "The integer 2 added to unity makes the integer 3, but when added to 3 it makes an amount greater by 2 and this.... is the next odd integer, namely 5. Again, since the integer 3 added to 2 makes 5, which is the collateral [third] odd integer, when it is added to 4 the result will be greater by 2, that is....it will be the next odd integer, which is 7. And in like manner to infinity as the proposition states." On this proof Bussey remarks (p. 201): "This is not a very clear statement of a proof by mathematical induction but the idea is there." The eleventh proposition, that every triangular number added to the preceding triangular number is equal to the collateral square number, or, in modern notation, $n(n+1)/2+(n-1)n/2=n^2$, is the one which Cantor said, in the above note, Pascal got from Maurolycus and which Maurolycus proved by complete induction. But Cantor is mistaken in saying that this theorem is proved by complete induction: the first undoubted case of a proof by complete induction is the fifteen proposition, that the sum of the first n odd integers is equal to the nth square number. Maurolycus's proof is (p. 203): "By a previous proposition the first square number (unity) added to the following odd number (3) makes the following square number (4); and this second square number (4) added to the third odd number (5) makes the third square number (9); and likewise the third square number (9) added to the fourth odd number (7) makes the fourth square number (16); and so successively to infinity...." Pascal mentioned in a letter to Carcavi the fact that he borrowed from Maurolycus, and he repeatedly used the method of complete induction in connection with his arithmetical triangle and its applications. Bussey then gives two interesting examples of Pascal's use of the method of complete induction, and finally gives some other and more recent uses of it.

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In the same number of the *Monthly*, David Eugene Smith ("Mathematical Problems in Relation to the History of Economics and Commerce," pp. 221-223) maintains that "a very good history of civilization could be written from the wide range of problems of mathematics." In the subject of commercial and economic his-

tory, for example, he mentions that the problems in the manuscripts and early printed books on arithmetic in the fifteenth century tell us that Venice was then the center of the silk trade, although Bologna, Genoa, and Florence were then prominent; the problems also tell us the cost of the luxuries and necessities of life; the rent of houses; the changes in commercial customs and the rise in standards of business integrity. "Not only to the economist and the student of commerce is the field a rich one, but it is well worth the study of any one who may be possessed of doubt as to the relation of mathematics to the daily life of the race. Not only can the history of the problem easily be made the history of commerce and economics, but the history of mathematics can easily be made the history of civilization."

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In the number of the *Bulletin of the American Mathematical Society* for March, 1917 (Vol. XXIII), there are two interesting papers by Edward V. Huntington on the logical postulates for order. In "Complete Existential Theory of the Postulates for Serial Order" (pp. 276-280) Huntington establishes the "complete independence"—in the sense defined by E. H. Moore of Chicago in his *Introduction to a Form of General Analysis* of 1910—of each of three different sets of postulates for serial order. The first set is new and very convenient for many purposes; the second set dates back to Vailati (1892); the third set is a modification of the second set and was introduced in Huntington's well-known paper on "The Continuum as a Type of Order" in the *Annals of Mathematics* for 1905. In "Complete Existential Theory of the Postulates for Well Ordered Sets" (pp. 280-282) Huntington gives three sets of independent postulates for well-ordered systems, each of these three sets being "completely independent" in the above sense. R. L. Borger ("A Theorem in the Analysis of Real Variables," pp. 287-290) gives a theorem on two real functions of two real variables which is derived from a theorem in Kowalewski's *Die komplexen Veränderlichen und ihre Funktionen,* and deduces from it the exceedingly fundamental and important theorem that if any function of a complex variable possesses a finite derivative at each point of a simply connected closed region, then this derivative is continuous, all the derivatives of the function exist, and the function may be represented by a power-series. Mathematicians who are acquainted with the nature of the progress brought about by Goursat's proof

of Cauchy's theorem will at once see how important this note is. J. R. Kline ("Concerning the Complement of a Countable Infinity of Point Sets of a Certain Type," pp. 290-292) proves a theorem which is a general case of the theorem proved by Hausdorff in his *Grundzüge der Mengenlehre* of 1914 that, if E denotes a Euclidean space of two or more dimensions while R is an enumerable set of points belonging to E, then E – R is a connected set. Kline's theorem was proved by Robert L. Moore (*Trans. Amer. Math. Soc.* for 1916) on the basis of a system of axioms proposed by him.

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The number of the *Bulletin of the American Mathematical Society* for May, 1917 (Vol. XXIII, No. 8), contains several articles of interest to those who cultivate the philosophical and historical aspects of mathematics. Samuel Beatty ("The Inversion of an Analytic Function," pp. 347-353) proves the existence of the inverse of an analytic function when the conception of an analytic function which is due to Goursat is the starting-point. In the theory of Weierstrass this proof is made to depend on the representation by a series of powers and in Cauchy's theory on the Jacobian of the real and imaginary parts of the function with reference to the real and imaginary parts of the variable. It is well known that Goursat showed in 1900 how the fundamental proposition on complex integration in Cauchy's theory could be proved merely from the assumption that the function in question has a finite derivative at each point of a simply connected domain, without any assumption of the continuity of this derivative. This continuity was then proved as a consequence of the Cauchy-Goursat theorem. The method of Beatty's proof makes use of the theory of sets of points. Thomas S. Fiske ("Emory McClintock," pp. 353-357) gives a biography of Emory McClintock (1840-1916). McClintock's first paper on pure mathematics entitled "An Essay on the Calculus of Enlargement," in the *American Journal of Mathematics* for 1879 "was an effort to present the theory of finite differences and the differential calculus from a unified point of view. The paper may be regarded as a precursor of recent attempts to consider difference equations as differential equations of infinite order. His other more important papers were a series of researches on solvable quintic equations published in the *American Journal of Mathematics* [for 1884, 1885 and 1898] and a paper on the theory of numbers ['On the Nature and Use of the Functions Employed in the Recognition of Quad-

ratic Residues'] published in the third volume [1902] of" the *Transactions of the American Mathematical Society* (p. 355). "When one considers that McClintock made no use of the powerful labor-saving machinery which has revolutionized modern analysis, the results obtained by him in his researches on quintic equations, as well as some of his other achievements, appear to indicate a truly wonderful power of manipulation and clearness of vision" (p. 356). A list of McClintock's mathematical publications is given.

J. H. Weaver ("On Foci of Conics," pp. 357-365) gives (1) a short historical sketch of the development of the properties of conics connected with the foci, and (2) some of the theorems from Pappus which have a bearing on foci and tangents. According to Zeuthen (*Geschichte der Mathematik im Alterthum und Mittelalter*, Copenhagen, 1896, p. 211) it seems that the focus for the parabola may have been known to Euclid. However, we have no mention of such points or of any of their properties until the work of Apollonius on conics (Book III, Probs. 45-52), but Apollonius did not use or mention in any way a focus for the parabola. Pappus gave the first recorded use and proofs of the focus-directrix definition of conics. Johann Kepler named the points in question in a work of 1604, and part of the short account of the conic sections that he gave (*Opera Omnia*, ed. Frisch, Frankfort, 1859, Vol. II, p. 185) is freely translated by Weaver (p. 359) as follows: "There are among these curves certain points of especial consideration, which have a certain definition but no name, unless they usurp for name the definition of some property. For if from these points lines are drawn to the points of contact of tangents to the section, these lines make equal angles with the tangents.... We, because of the properties of light and the eye, from the viewpoint of mechanics shall call these points *foci*. We might have called them *centers*, because they are on the axis of the section, if authors, in the hyperbola and ellipse, were not accustomed to calling another point the center. In the circle there is one focus, the center. In the ellipse there are two foci equally distant from the center, and more removed in the more acute. In the parabola, one focus is within the section and the other may be considered either within or without the section and removed to an infinite distance from the first focus, so that if a line drawn from this *caecus* [blind] focus to a point of the section will be parallel to the axis. In the hyperbola, the external focus becomes nearer the internal focus as the hyperbola becomes more

obtuse." The method of the work of Kepler was developed and added to by Desargues, and important work on foci was done by Maclaurin, Poncelet, Plücker, and many others.

Finally, there is a short and extremely interesting paper by Jekuthial Ginsburg ("New Light on Our Numerals," with an introductory note by David Eugene Smith, pp. 366-369). That our common numerals are of Hindu origin seems to the author to be a well-established fact, and that Europe received them from the Arabs seems equally certain, but how and when these numerals reached the Arabs is a question that has never been satisfactorily answered. The article calls attention to a paper by the French orientalist F. Nau in the *Journal asiatique* for 1910 (Series X, Vol. XVI) showing that the Hindu numerals were known to and appreciated by the Syrian writer Severus Sebokht who lived in the second half of the seventh century; that is, about one hundred years before the date of the first definite trace that we have hitherto had of the introduction of the system into Bagdad. Sebokht says, after asserting that the Greeks, in astronomy, were merely the pupils of the Babylonians: "I will omit all discussion of the science of the Hindus, a people not the same as the Syrians; their subtle discoveries in this science of astronomy, discoveries that are more ingenious than those of the Greeks and the Babylonians; their valuable methods of calculation; and their computing that surpasses description. I wish only to say that this computation is done by means of nine signs. If those who believe, because they speak Greek, that they have reached the limits of science should know these things they would be convinced that there are also others who know something" (p. 368). On this fragment Ginsburg remarks (pp. 363-369) that it "clearly shows that not only did Sebokht know something of the numerals, but that he understood their full significance, and may even have known the zero as Rabbi ben Esra did, in spite of the fact that he, too, speaks of nine numerals." However, Smith (pp. 366-367) remarks that the article "shows that the zero was probably not in the system as then mentioned, showing at least that its value was not generally comprehended in the seventh century and possibly confirming the impression that the symbol had not yet been invented." With regard to the question as to how Sebokht could have obtained information about the Hindu numerals, Ginsburg remarks (p. 369) that the city where Severus lived, in the northeast part of Mesopotamia, "was situated in a rich and fruitful country, was long the

center of a very extensive trade, and was the great northern emporium for the merchandise of the east and west;" and "the exchange of goods is always accompanied by the exchange of ideas." Further, the weight of the evidence is (p. 369) in favor of Sebokht's work being at least one of the agencies by means of which the knowledge of the numerals was transmitted to the Arabs.

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Raphael Demos ("A Discussion of a Certain Type of Negative Proposition," *Mind,* Vol. XXVI, 1917, pp. 188-196) applies to particular negative propositions the treatment which Bertrand Russell (cf. Russell and Whitehead's *Principia Mathematica,* Vol. I, Cambridge, 1910) has applied to "descriptive phrases" or "incomplete symbols." Russell "found himself confronted with the fact that to accept descriptive phrases as significant in their given form would be to people the world of things with the apparent objects of such self-contradictory and fantastic descriptions as 'round-square,' 'centaur,' etc."; and Demos "was faced with the fact that to accept negative propositions at their face value would be to people the world of objects with negative facts, a type of objects which experience fails to disclose." Demos, somewhat like Russell, by viewing the negative proposition as an incomplete symbol, was led to declare it meaningless in its apparent form, and its apparent object —the negative fact—to be nothing. In this article he stated, first, that a particular simple negative proposition is an objective entity whose peculiarity as negative is not dependent upon the mind's attitude toward it. He then argued that the negative proposition cannot be construed in the form which it apparently possesses, inasmuch as such construction would make it formally different from positive propositions and would endow it with purely negative objects, which are, it seems, nowhere to be found in experience. He concluded that some special interpretation must be given to the negative proposition, and showed that its negative element is a modification, not of any distinct constituent (such as the predicate) in the proposition, but of the whole content of it. Thus any negative proposition is a modification, in terms of "not," of the rest of its content, and—since the latter is positive—a modification of some particular positive proposition. He stated the meaning of "not" to be "opposite"—a relational qualification in terms of the familiar relation of opposition or contrariety among positive propositions—and hence the meaning of the whole proposition "not-p" to

be "opposite of p." He argued that, so stated, a negative proposition is an ambiguous description of some positive proposition, and that, completely stated, it is of the form "an opposite of p is true," or "some q is true which is an opposite of p." Thus he defined a particular simple negative proposition as an ambiguous description of some true positive proposition in terms of the latter's relation of opposition to a certain other positive proposition, such that, in terms of the former, reference is achieved to the latter. Lastly, he explained that negative knowledge is knowledge of a true positive proposition by description in terms of its opposition to some other proposition, and hence must be characterized as positive in reference but not in content, inasmuch as the proposition referred to is not a constituent of the complex of assertion or knowledge. "Substantially the above definition of simple negative propositions applies to double and 'n-ple' negatives as well; the latter, too, are descriptions of positive propositions which are true in terms of what they oppose. There is this difference, however, that whereas simple negatives are functions of a positive content, double and other negatives are functions of a negative content, such that any negative proposition in the nth power is a function of a content which is negative in the $(n-1)$th power." Φ

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C. E. Hooper publishes "The Meaning of the Universe" (*Mind*, April, 1917), the first instalment of an article of massive appearance. The definition is as follows: the *Universe* means the totality of real thought-objects (or object-matters) considered under four related aspects: (1) space, (2) time, (3) the variety in unity of natural characters, i. e., real thought-objects as particulars having natures of their own, but natures agreeing in various specific and generic respects with the natures of other particulars, (4) unity in variety of natural causation. Time and space are both objective. Mr. Hooper goes on to define thought-object, reality and aspect. A thought-object is apparently an *intended* object, whether or not a reality corresponds to the intention (e. g., Kant's *noumenon*). Reality is contrasted not with appearance but with "mental figment," and includes subsistent as well as existent objects. It is difficult to tell how far the term "thought-object" has an idealistic bias in Mr. Hooper's mind, but reality, at all events, seems to be merely a sum or system of objects which are severally real. The universe is thus a real thought which contains all other real thought-objects in their

manifold relations. *Symbolic* entities (ideas, signs) are comprehended, but whether *per se* or only as reflected upon (made objects of thought) is not stated. It would seem that imaginary or inconceivable thought-objects, such as Meinong's pets, the golden mountain and the round square, are to have no place in the universe, but are discarded as "figments." While the universe contains finite thought-objects and symbols, it does so only in fact, not in nature. Of the four modes or aspects, space and time may be classed together as *coincidentals,* while the systems of natural characters and natural causation may be termed *co-essentials.* On the other hand space and nature may be classed together as static, time and causation as dynamic. We find some difficulty in understanding how Mr. Hooper accounts for the universe's being known at all. "It cannot, like a finite object, be actually related to some fellow object. It is as related to the mind or system of subjective ideas that we know all that is possible to know about it." But the "mind" if genuinely symbolic is a thought-object; and if the universe cannot be related to a finite object which is part of itself we do not know how it can be related to the mind. But criticism of so substantial an article should be deferred until its completion in succeeding issues. η

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Agnes Cuming ("Lotze, Bradley, and Bosanquet," *Mind,* April, 1917) declares Lotze's logic to be a partial revolt against the intellectualism of Hegel. Our intelligent experience, according to Lotze, is only a small part of the real world and thought is only a small part of our intelligent experience. Thought is a tool, a substitute for adequate perceptive intuition. Bradley's and Bosanquet's logic are similar in so far as each is influenced by Lotze. They hold an almost identical definition of "idea," and agree in their theories of judgment. Bradley however arrives at reality ontologically and Bosanquet epistemologically. "Knowledge for Bosanquet is the system of reality progressively demonstrated before our eyes.... In this emphasis on system as the postulate of knowledge....Bosanquet is in advance of Bradley." Lotze insists on feeling as a criterion, and is thus very far from Bosanquet with his conception of system, but he admits the essential of Bosanquet's position, which is the inadequacy of feeling. Lotze is a dualist: he divides sharply the feeling which supplies the material from thought, exercising a formal activity upon it. In Bradley the dualism becomes a gloomy

scepticism; thought and its object are forever sundered. But Bosanquet bridges the gulf. In both Bosanquet and Bradley the separation of thought and reality is inherited from Lotze with his idea of the "scaffold" of thought. The only possible criterion of knowledge is immanent—a criticism of a lower from a higher point of view. Miss Cuming considers that Bosanquet has improved upon both Lotze and Bradley; the direction which she believes to be progress seems to be almost a return to more orthodox Hegelianism. ¶

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In the same number B. M. Laing ("Schopenhauer and Individuality") considers that Schopenhauer fails to appreciate the metaphysical claims of individuality. He dethrones reason, making it a mere temporary organ of the will. He interprets Kant in such a way as to make Kant assert that the mind creates the world of things, instead of merely conditioning it. This perversion of the Kantian doctrine leads Schopenhauer to hold (in contrast to Kant) that the world of space and time is an illusion. Hence he is unable to conserve individuality, and tends to confuse individuality with (temporal and spatial) individuation. Schopenhauer's monism is a mere prejudice against multiplicity, and his will a pure abstraction. Furthermore, he confuses the will with bodily wants and cravings. Schopenhauer exposes himself on every side to such destructive criticism: but while Mr. Laing seizes upon some of his weakest points in his interpretation of Kant, the view of individuality which Schopenhauer represents, and which is more abiding then Schopenhauer, cannot be said to be demolished. ¶

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Scientia for February, 1917, opens with an article by Gino Loria on the history of imaginary numbers. He takes as his text Kronecker's aphorism "Die ganzen Zahlen hat der liebe Gott gemacht, alles andere ist Menschenwerk." There is no square root of a negative quantity, said the Viga Ganita, for it is not a square, and the mysterious quantities remained an enigma and defied concrete interpretation until a memoir appeared about the end of the eighteenth century, written by an unknown Danish land-surveyor, Caspar Wessel by name. It seems to have suffered a fate like that of Swinburne's Queen Rosamund, of which not a copy was asked for or sold. Mendel's discovery remained unheeded for forty years, but Wessel's was not unearthed until a century after his death. But in

this summary is no more room for comment on Loria's charming paper. P. Zeeman writes upon the hypothesis of the immovable ether, describing the experiments of Fizeau, Michelsen and Morley, Eichenwald, and himself, and concluding that it seems impossible by any imaginable means to measure absolute velocities. He points the lesson that the most important scientific principles are the results of boldly generalized experiments. J. R. Carracido discusses the foundations of biochemistry, and the lines research must follow in the pursuit of organic synthesis. He does not see why success should not be reaped before the end of the century, and success it will be, even if limited to the synthesis of the most rudimentary form of living matter.

The number for March, 1917, contains an admirable article by Gaston Milhaud, in which he attacks the very difficult problem as to the extent to which Descartes was influenced by Bacon. M. Cantone discusses the present trend of physical research in a paper surveying the work of those whose discoveries have in thirty or forty years revolutionized our outlook on the world of matter. Etienne Rabaud writes on the life and death of species. An analysis of the current doctrine of "means of defense" prepares for the question as to how it is that species persist in spite of the daily hecatombs of individuals. γ

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In *Scientia* for April, 1917, we have, from the pen of Francisco Iñiguez, of the Observatory of Madrid, a slight but interesting sketch of what we know about stellar spectra, their classification, and the light they throw on the subject of the evolution of the stars. We are warned of the limits we must set to our inferences in considering the nebulae, for what we know as yet of these celestial bodies does not justify our indulging in theories on the subject. The author then indicates how we may infer the existence of dark stars, how their evolution still continues, and points out their connection with meteorites and cosmic dust. Etienne Rabaud brings to a close his paper on the life and death of species, of which this second instalment deals with the conditions of the persistence and of the disappearance of species. He finds the affinity of organisms to be the crux of the problem. This leads to the consideration of the conditions of attraction and repulsion, and to a short discussion of parasitism and symbiosis, with the cosmic influences which, often of great complexity, determine the life of a species. The whole

forms a graphic picture of the variations of the relative proportion of individuals and species. The slightest change in the conditions of normal life may lead to the disappearance of the last member of a species, or on the contrary the species may thrive and continue to thrive. Species persist, they increase immeasurably in numbers, or their numbers fall off, and they disappear, subject but to the intervention of two sets of influences, affinity and the circumstances that determine their displacements in space. "These modifications, no doubt, sometimes involve other important modifications in the conditions of life; variations may ensue which find their repercussion in the aggregate of the interaction. Thus, linked to one another and to the world from which they come, the life, the transformations, and the death of organisms are functions of their interdependence." γ

BOOK REVIEWS AND NOTES.

DIDEROT'S EARLY PHILOSOPHICAL WORKS. Translated and edited by *Margaret Jourdain*. "The Open Court Classics of Science and Philosophy," No. 4. Chicago and London: The Open Court Publishing Co., 1916. Pp. vi, 246. Price $1.25 or 4s. 6d. net.

Among the documents of the renaissance of the eighteenth century, none are of more interest than the early informal contributions to ethics and philosophy of Diderot, written with much of the incoherence of the epistolary form. They are, as he claims again and again, *Letters*; and they are letters written in a hurry. *The Philosophic Thoughts*, which is the only one of Diderot's works in this selection not in the epistolary form, is said to have been thrown together between Good Friday and Easter Monday of 1746. Yet they are not philosophic journalism, no mechanical transmission of the current philosophical coin of the day. It is for their originality of outlook that they have been closely studied in Germany, while in England there is Lord Morley's study of Diderot in relation to the movement centered in the *Encyclopédie, Diderot and the Encyclopædists*.

This selection includes the *Philosophic Thoughts*, a breviary of eighteenth-century scepticism, a copy of which was found in the possession of the unfortunate La Barre, and in which Diderot appears as a Deist, to whom the argument from design (Thought XX, pp. 56-58) is still of weight: "I am greatly deceived (he writes) if this proof is not well worth the best that has ever issued from the schools." That very argument is very differently treated in the *Letter on the Blind* (p. 109) by Diderot's mouthpiece, the blind mathematician, Nicholas Saunderson, who conjectures a world in its early stages "in a state of ferment," without any vestiges of that "intelligent Being whose wisdom fills you with such wonder and admiration here.... What is our world, but a complex, subject to cycles of change, all of which show a continual

tendency to destruction; a rapid succession of beings that appear one by one, flourish and disappear; a merely transitory symmetry and momentary appearance of order?"

In the brilliant passages in which Diderot sketches the probability of evolution he appears as a forerunner of thinkers such as Erasmus Darwin in England and Lamarck in France. Transformism only needed the partial scientific confirmation it received from Lamarck and Geoffroy St. Hilaire in the early decades of the nineteenth century, "to pass from the realm of systematic philosophy into that of scientific controversy."

The *Letter on the Deaf and Dumb*, a criticism addressed to the Abbé Batteaux, author of the *Fine Arts Reduced to a Single Principle*, has its interest as a forerunner of Lessing's *Laokoon*, in esthetics. It also contains the idea of a *muet de convention* (theoretical mute), which is closely paralleled by Condillac's Statue in the *Treatise on the Sensations*, published three years after Diderot's Letter. Condillac's treatment of the idea, however, was far more systematic and detailed than Diderot's, and he did not by his own account owe the suggestion of his statue to Diderot.

Diderot, the most German of French authors, as far as his style is concerned, bears translation well. He has been neglected by translators, however, until this edition, which includes all that is of permanent value in his early works of 1751, the date of the Letter on the *Deaf and Dumb*, excluding the relatively uninteresting *Sceptic's Walk*.

THE NEW PHILOSOPHY OF HENRI BERGSON. By *Edouard Le Roy*. Translated from the French by *Vincent Benson, M.A.* New York: Holt. Pp. 235. Price $1.25 net; by mail $1.35.

This interpreter of Bergson's philosophy is also the author of the article "What is a Dogma?" in the body of this issue of *The Monist*. He is particularly fitted for the present task because though not a pupil of Bergson's he had followed much the same trains of thought quite independently so that when he became acquainted with Bergson he recognized in his work, as he himself says, "the striking realization of a presentiment and a desire." That M. Le Roy has comprehensively grasped Bergson's spirit and conclusions so that the present volume furnishes a valuable *prolegomenon* to the study of the famous Frenchman's thought is attested by the following lines in the Preface in which Bergson himself has set the seal of his approval on the task. M. Bergson wrote to M. Le Roy: "Underneath and beyond the method you have caught the *intention* and the *spirit*.... Your study could not be more conscientious or true to the original. As it advances, condensation increases in a marked degree: the reader becomes aware that the explanation is undergoing a progressive involution similar to the involution by which we determine *the reality of Time*. To produce it is feeling, much more has been necessary than a close study of my work: it has required deep sympathy of thought, the power, in fact, of rethinking the subject in a personal and original manner. Nowhere is this sympathy more in evidence than in your concluding pages, where in a few words you point out the possibilities of further developments of the doctrine. In this direction I should myself say exactly what you have said."